S. Hrg. 101-14

PROPOSED FISCAL YEAR 1990 BUDGET REQUEST

(DOE's Office of Energy Research)

HEARING

BEFORE THE

SUBCOMMITTEE ON
ENERGY RESEARCH AND DEVELOPMENT

OF THE

COMMITTEE ON
ENERGY AND NATURAL RESOURCES
UNITED STATES SENATE

ONE HUNDRED FIRST CONGRESS

FIRST SESSION

ON THE

DEPARTMENT OF ENERGY'S FISCAL YEAR 1990 BUDGET REQUEST FOR THE OFFICE OF ENERGY RESEARCH

FEBRUARY 24, 1989

Printed for the use of the
Committee on Energy and Natural Resources

U.S. GOVERNMENT PRINTING OFFICE
96-148 WASHINGTON : 1989

For sale by the Superintendent of Documents, Congressional Sales Office
U.S. Government Printing Office, Washington, DC 20402

S311-37

COMMITTEE ON ENERGY AND NATURAL RESOURCES

J. BENNETT JOHNSTON, Louisiana, *Chairman*

DALE BUMPERS, Arkansas
WENDELL H. FORD, Kentucky
HOWARD M. METZENBAUM, Ohio
BILL BRADLEY, New Jersey
JEFF BINGAMAN, New Mexico
TIMOTHY E. WIRTH, Colorado
KENT CONRAD, North Dakota
HOWELL T. HEFLIN, Alabama
JOHN D. ROCKEFELLER IV, West Virginia

JAMES A. McCLURE, Idaho
MARK O. HATFIELD, Oregon
PETE V. DOMENICI, New Mexico
MALCOLM WALLOP, Wyoming
FRANK H. MURKOWSKI, Alaska
DON NICKLES, Oklahoma
CONRAD BURNS, Montana
JAKE GARN, Utah
MITCH McCONNELL, Kentucky

Daryl Owen, *Staff Director*
D. Michael Harvey, *Chief Counsel*
Frank M. Cushing, *Staff Director for the Minority*
Gary G. Ellsworth, *Chief Counsel for the Minority*

Subcommittee on Energy Research and Development

WENDELL H. FORD, Kentucky, *Chairman*
JOHN D. ROCKEFELLER IV, West Virginia, *Vice Chairman*

DALE BUMPERS, Arkansas
HOWARD M. METZENBAUM, Ohio
TIMOTHY E. WIRTH, Colorado
HOWELL T. HEFLIN, Alabama

PETE V. DOMENICI, New Mexico
MITCH McCONNELL, Kentucky
DON NICKLES, Oklahoma
CONRAD BURNS, Montana
JAKE GARN, Utah

J. Bennett Johnston and James A. McClure are Ex Officio Members of the Subcommittee
Benjamin S. Cooper, *Senior Professional Staff Member*

CONTENTS

Page

STATEMENTS

Anderson, Dr. Philip W., Joseph Henry professor of physics, department of physics, Princeton University 132
Barton, Hon. Joe, U.S. Representative from Texas 2
Bentsen, Hon. Lloyd, U.S. Senator from Texas 18
Ford, Hon. Wendell H., U.S. Senator from Kentucky 7
Gramm, Hon. Phil, U.S. Senator from Texas 10
Heflin, Hon. Howell T., U.S. Senator from Alabama 216
Hunter, Dr. Robert O., Director, Office of Energy Research, Department of Energy 26
Schwitters, Dr. Roy F., director, SSC Laboratory 90

APPENDIXES

Appendix I

Responses to additional questions 219

Appendix II

Additional material submitted for the record 287

PROPOSED FISCAL YEAR 1990 BUDGET REQUEST

(DOE's Office of Energy Research)

FRIDAY, FEBRUARY 24, 1989

U.S. SENATE,
SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT,
COMMITTEE ON ENERGY AND NATURAL RESOURCES,
Washington, DC.

The subcommittee met, pursuant to notice, at 10:05 a.m., in room SD-366, Dirksen Senate Office Building, Hon. Wendell H. Ford, presiding.

OPENING STATEMENT OF HON. WENDELL H. FORD, U.S. SENATOR FROM KENTUCKY

Senator FORD. Today the Subcommittee on Energy Research and Development begins its hearings on the proposed fiscal year 1990 Research and Development Budget of the Department of Energy. Our first hearing will be devoted to the proposed budget for the scientific research supported by DOE—physics, chemistry, mathematics, biological and environmental science and climate research.

Perhaps the most famous proposal in that budget is the superconducting super collider, a high-energy physics particle accelerator 53 miles around that is going to cost at least $6 billion. It is the biggest thing we are proposing to build in the civilian sector, except for the space station.

We are going to hear a status report on the SSC. The site has now been chosen in Texas. A project manager and director have been selected, but Congress has yet to authorize construction.

Most of us are still wondering where the money will come from. We know it will not come from "let's pretend" sources in the Reagan budget. President Bush has an opportunity to make a break with the dead-on-arrival presidential budgets of the past 8 years. He needs to tell us how we are going to pay for the wonderful things he is proposing.

This is a good place to start. President Reagan proposed this accelerator project and said it would be funded from "new" money. No money would be taken out of any other areas of scientific research, but the budget would be reduced. Taxes would not be raised.

Congress was left to point out that this arithmetic just does not work, but the President gets credit for being for science and progress and reaching for the stars. Congress gets blamed for being too short sighted to provide the money.

It has been a good deal for President Reagan and it has been a bad deal for science. Priorities have not been established and projects have not been built. Building things in the present budget environment takes hard work to develop the support, find the revenues, and make the tough tradeoffs.

The Reagan Administration was not willing to do that hard work and they did not get anything built. Unless the Bush Administration is willing to face up to the arithmetic, they are not going to get anything built either.

The losers in that event will be the scientific community. We will not have what the President proposes because we cannot afford it, and we will not have a sensible science policy either, only a lot of fancy, expensive proposals that look good in press releases but do not move our understanding of nature forward even one little inch.

Senator Burns, do you have a statement this morning or any comment you would like to make?

Senator **Burns**. I do not, Mr. Chairman.

Senator **Ford**. We now have Congressman Joe Barton from the great State of Texas' Sixth Congressional District, and Joe, we would be delighted to hear you this morning and here comes Senator Gramm. Since you were here first, I will let you go first, and then we will let the two senators go because you are outnumbered this morning two to one. So if you want to proceed, you may go ahead.

STATEMENT OF HON. JOE BARTON, U.S. REPRESENTATIVE FROM TEXAS

Mr. **Barton**. Thank you, Senator Ford and Senator Burns. It is a pleasure to testify before your committee this morning. I enjoyed working with you and other members of the committee in the last Congress to pass some legislation on the natural gas issue, and I look forward to working with you and the other members to pass this authorization bill on the SSC in this Congress.

I would like to point out that we are now at the stage for the SSC where we need to make a commitment to construction. We started the process in 1983 in the scientific community. There was a site selection process that began two years ago that initially involved more than 25 states.

In November of last year the Secretary of Energy at that time, John Herrington, announced the preferred site, which has now become the official site for the SSC. It is in my Congressional District. It is in my home county, and it is literally in my backyard of Ellis, Texas.

Today's hearing begins the debate on the construction and funding of this project. The SSC will be one of the most important and vital research projects every undertaken by the United States of America. I would urge you and your committee members to authorize full construction funding for the project this year.

Since the emergency of high-energy physics as a separate field of research, the United States has led the world. I am concerned, however, that we may begin to lose our preeminence in this field, first to Europe with their Center for European Research-Nuclear, called the CERN, and perhaps to the Soviet Union with their planned collider which I believe they call the UNK. If we build the SSC in the United States we will maintain our leading position.

Since 1975, American scientists have won half of all the Nobel prizes in physics. Unfortunately, the last eight of those prizes have been won by Europeans. If we lose our leadership position in high-energy physics, we will ultimately lose our leading position in the world economic situation today. If the SSC is not constructed in the United States, future U.S. generations will be forced to buy the technologies of tomorrow from the nations that are willing to invest in the SSC today.

Fully one third of our Gross National Product today is developed from technologies that were originated by research in high-energy physics. I have seen estimates that if we do construct the SSC by the year 2000 we could have additional markets of over $10 billion for spin-off technologies that will result from construction of the SSC.

Now, there are those that claim that the SSC will adversely affect other research and development projects. You alluded to that in your opening remarks. In my opinion, the reverse is true. Areas such as superconductivity, photonics, robotics and supercomputing will receive increased emphasis because of the SSC. The SSC is the bellwether project for investment in areas of basic research in the hard science area and will provide a major boost to the U.S. economy.

As you know, the President has advocated doubling the National Science Foundation budget. I support this increase. The United States must commit the resources to assure initiatives for the continued economic health of future generations. The SSC is not part of the NSF increase, but it is a complement to that effort. Basic research and development is a valid mission for the federal government to be involved in, and the SSC and the many NSF projects are a major portion of our Nation's basic research budget.

Let us talk a little bit about the cost. You indicated it is going to cost $6 billion to construct. That is true. That is what the construction commitment will be. However, that is not all. The federal share, the federal share for this year is expected to be $250 million.

Let us put that into perspective. The federal budget is going to spend $1,150 billion in the fiscal year that we are now debating. You break that down, that is $3 billion a day, or about $125 million an hour. We are asking the federal government to spend two hours worth of our resources in the next fiscal year on building the SSC. Two hours to maintain our preeminent position in the world physics research budget for the world economy. I think that is a small price to pay.

Of this total of $6 billion, approximately $4.3 billion is federal funding, the remainder is going to come from what they call non-federal sources. The voters of the State of Texas voted two years ago to spend $500 million in general revenue bond money from the state if the SSC was built in Texas. The State Legislature authorized another $500 million worth of bonds, so that is $1 billion that the state taxpayers of the State of Texas are willing to put on the table if we build the SSC.

Many foreign governments have shown great interest in cost-sharing. One has actually signed a document. The government of India has signed a working agreement with the Department of Energy that if we construct the project they will put up to $50 million into it, and other governments such as the Italian government, the Korean government, the Japanese government, the Taiwanese government, the West German government, have had at least low-level talks with the Department of Energy officials on cost-sharing. So although it will cost $6 billion that you indicated, only about two thirds of that is going to come from federal sources. Up to one third will come from nonfederal sources.

In order to begin the project, though, we need to obviously authorize construction this year. As I indicated earlier, we have been working on this since 1983. The research program is going well. This is the year to make a commitment. As in any large-scale project there is a lag time, and we need to authorize construction this year in order to get the project on line by the year—between 1996 and the year 2000.

To end, Mr. Chairman, I would like to quote from a Nobel Prize winner from the University of Chicago, James W. Cronin: "The spirit of a nation and the pride of its people can only be enhanced when science, including the exploration of our planet, solar system, galaxy, and universe, is among its highest priorities."

This is a high priority. I would urge you and the committee members to vote for full funding and construction authorization of the project this year. Thank you.

[The prepared statement of Mr. Barton follows:]

TESTIMONY
OF
CONGRESSMAN JOE BARTON
TEXAS, SIXTH DISTRICT

SENATE ENERGY & NATURAL RESOURCES COMMITTEE
HEARING ON
DEPARTMENT OF ENERGY'S FY 1990 REQUEST FOR
BASIC ENERGY RESEARCH PROGRAMS AND THE SSC

FEBRUARY 24, 1989
336 DIRKSEN

"SUPERCONDUCTING SUPER COLLIDER: A NATIONAL COMMITMENT"

Mr. Chairman, I would like to thank you and the Committee for giving me the opportunity to testify today. I wish to testify on behalf of the Superconducting Super Collider (SSC). The SSC Program has received a great deal of national media attention during its two year site selection process. This competitive evaluation initially involved more than 25 states.

As you know, in November of 1988, former Energy Secretary John Herrington announced that the "preferred," and now "official" site for the construction of the SSC is Ellis County, Texas. Not only is this Ellis County SSC site in my home State of Texas, but also in my Congressional District, with part of the proposed ring almost literally located in my own backyard.

Today's hearing begins the Congress' debate on the construction and funding of this project. The SSC will be one of the most important and vital research projects ever undertaken by the United States. It will be the world's most powerful proton-proton collider, producing particle collisions with total energies approaching 40 trillion electron volts, an energy twenty times the highest energies available in existing particle accelerators. I, along with 271 of my House colleagues cosponsored legislation which authorized the project during the 100th Congress. I urge you and your Committee to authorize full contruction funding for the SSC this year.

Since the emergence of high energy physics as a separate field of research, the United States has led the world. However, I am concerned about losing our preeminence in high energy physics, first to Europe with the Center for European Research-Nuclear (CERN) and perhaps to the Soviet Union with their planned collider called the "UNK." By building the SSC, the United States will maintain its leading position.

Understanding the nature of the world around us will provide research advances of literally cosmic proportions. Funding the Super Collider allows the United States to remain in the forefront of basic science technology for many years to come. Since 1975, American scientists have won half of all Nobel Prizes in physics; however, Europeans have won the last eight. This is a clear indication we are losing our leadership position in high energy physics. If we ultimately lose our leadership position in high energy physics, we will lose our leading edge in world economics.

The SSC will be used by research scientists from around the world and will assure our technological leadership in high-energy physics. If the SSC is not constructed in the United States, future U.S. generations will be forced to buy the technologies of tomorrow from the nations willing to invest in the SSC today.

The development of the SSC will usher in a new era in science and technology, much as the discovery of the electron did 100 years ago. The Super Collider will quite probably provide technical innovations, educational discoveries, new medical treatments and commercial spinoffs undreamed of today. Currently, fully one third of the production making up our gross national product is based on the knowledge of the atom. An investment in the SSC should increase this percentage in the future. I have seen estimates that if the SSC is constructed, superconductivity products could well generate a market of over $10 billion by the year 2000.

Some claim the SSC will adversely affect other research and development projects. Just the reverse is true. Areas such as superconductivity, photonics, robotics and supercomputing will receive increased emphasis because of the SSC. The SSC is the bellweatner project for investment in areas of basic research in the "hard" science area and will provide a major boost to the U.S. economy.

The President has advocated doubling the National Science Foundation budget by 1990. I support this increase. The United States must commit the resources to assure initiatives for the continued economic health of future generations. The SSC is not part of this NSF increase, but it is a compliment to that effort. Basic research and development is a valid mission for the federal government to be involved in, and the SSC and the many NSF projects are a major portion of our nation's basic research effort.

As you know, the expected cost of the SSC is $5.9 billion to construct. However, since SSC funding is spread out over many years, the total funding outlay in any given year is relatively modest. The FY 1990 budget request is for a mere $250 million. Based on expected Federal expenditures of over 1.15 trillion dollars, this $250 million is two hours of expected annual Federal spending. When viewed in the overall Federal budget of $1.15 trillion, the funding for the SSC is small indeed.

The $250 million is a realistic figure which will keep the projected full industrial production of the magnets scheduled to begin in 1993 on time. In addition, this figure will give the needed U.S. construction commitment for foreign governments to actively negotiate potential and planned cost-sharing initiatives.

A substantial portion of the SSC funding can come from non-Federal sources. In my state of Texas, voters authorized $500 million in general obligation bonds to help pay for the SSC's construction should it be located in Texas. Texans voted for the bonds because they are dedicated to the United States' scientific excellence, and believe that scientific excellence is necessary to continue economic excellence. Both houses of the State Legislature and the Governor also voiced their commitment with an additional $500 million in funding which makes the total Texas funding package equal to some $1 billion.

Once we in the Congress commit to build the SSC, many foreign governments have expressed a serious interest in cost-sharing. India has agreed to $50 million, and Japan, Taiwan, Korea and several Western European nations have held preliminary discussions with American officials.

Under the current funding schedule, the project can be completed as early as 1996. To meet this ambitious time table, we must authorize the construction of the project this year. Please consider that direct benefits of basic research typically accrue to later generations (i.e. quantum mechanics was developed in the 1920's; the transistor followed in 1947. Nuclear magnetic resonance was discovered in the 1940's; commercial imaging medicine devices followed in the 1970's). We cannot afford to wait on authorizing construction of the SSC this year.

To quote a Nobel Prize winner from the University of Chicago, James W. Cronin: "The spirit of a nation and the pride of its people can only be enhanced when science, including the exploration of our planet, solar system, galaxy, and universe is among its highest priorities."

The SSC should be amongst our highest priorities. I urge this committee to fully support this essential project.

Senator Ford. Thank you, Congressman, and we will wait for any questions and listen to Senator Gramm, our colleague.

STATEMENT OF HON. PHIL GRAMM, U.S. SENATOR FROM TEXAS

Senator Gramm. Well, Mr. Chairman, let me first say I appreciate being here and I appreciate this hearing. I am happy to be here with my Congressman, Joe Barton.

Mr. Chairman, this project is about the future of America. Since the beginning of our country we have invested in science and technology and growth. From the colonial period we have had the highest real wages in the world. We have never been able to compete with foreign nations by doing it for less.

We have always had the most productive worker in the world. That worker has always been paid the highest wages, and we are committed to seeing that that is always the case. But that obligates us to do things better, and doing things better in the modern era means being the leader in developing technology. Investing not in the next election, but in the next generation, and I believe that is what the SSC is about.

Now, let me make it clear. Joe Barton is here and I am here in part because this project is in Texas. I made it clear when the Department of Energy was considering over 40 proposals that I was for the SSC and would vote for it no matter where it was put, but we are obviously involved in this for two reasons.

One, this is America's project but it is being built in Texas. Texas has an opportunity because our proposal was judged to be the best, to be the leader of the Nation in this area. But make no doubt about it, Mr. Chairman, this is not Texas' project, this is America's project.

The site selection was in Texas basically for two reasons: number one, the geological formations in the area basically are the finest formations in terms of stability and in terms of cost of construction of any area in the country.

Secondly, in looking at the site itself you had a relatively small number of landowners; you had massive public support. Texans want this project. We have been in a recession. We see this as being important to us as a source of jobs, but it is more than that.

This project is going to be the most important scientific project built anywhere in the world in the last quarter of the 20th Century, and I think it clearly is a source of pride to the people of Waxahatchie and the people of Texas that they have been selected as the site area.

But a second reason that I think is very relevant to the authorization and the appropriation process is that in the midst of a recession, the people of Texas voted in a Constitutional Amendment to put $1.1 billion on the table as our share of the construction and operation of this facility.

That is far more money than any other state was willing to put up to make this a reality, and that is important I think, because what it shows is that there is local support; that local taxpayers are willing to pay more taxes in order to make this project a reality, and that represented a commitment that was not matched anywhere else.

The GAO study I believe will show that the selection process involving the National Academies of Sciences and the National Academy of Engineering was a selection process that clearly was based on merit, and so I am not here, Mr. Chairman, to give you a scientific dissertation as to why this is a great project.

I see it as being important to science and medicine, to defense. I see it as being important to new technology and new growth. The benefits of this expenditure are not going to be felt immediately, but they will be felt for decades to come, in creating new technology which will heal the sick, new technology which will defend America, new technology which will create jobs and growth and opportunity in Texas, and jobs and growth and opportunity in America. New technology that will continue to make us the most productive Nation in the world, with the highest productivity and ultimately the highest wages for our workers.

So this is an investment in the future, and I think it is vitally important. It is part of the President's overall initiative that is aimed at trying to invest more in the future by investing in science and technology. That investment ranges from the space station to the National Science Foundation, to the SSC.

I support that initiative. That means setting priorities and that is difficult, and I understand that as well as anybody else. But I believe the future represents an important priority and it is one that I am willing to invest in, and I commend this project to the committee.

Senator **Ford**. I do not know that I have any questions. There might be one, that there is some hesitation among the scientific community whether we should move forward, where the Japanese have very little interest, if any, in the collider, SSC; where the Russians have a great deal of interest in this particular field.

The so-called atom-smashing scientists have what they refer to as an international brotherhood and they work together and publish openly, and the fruits of the research are available to all of them in that international field, and so they refer to us, our rushing forward with this, as a madcap competition which makes no sense. Do you have anything that you want to say as it relates to combatting that statement?

Senator **Gramm**. Mr. Chairman, let me respond to that by saying that first of all in the pure research areas, we do publish pure research and that research is available to anybody in the world, but the plain truth is that the scientific community and the nation that developed the pure research have an insurmountable lead in putting that research to practical use.

The early work on the atomic bomb was all published in research papers all over the world, and every scientist had access to it, but because we had Fermi and others that had done the research, we had a more fundamental understanding than did the Germans, and as a result we built the atomic bomb first.

In our basic defense areas the normal procedure is to have research open, because that is the only way it gets disseminated even among our own people, until you have the fundamental breakthrough theoretically, and then as it moves from a physics problem to an engineering problem, then it becomes proprietary with the company that is going to develop a new product. It becomes classified work for the Defense Department if it has defense applications.

I do not see that really as a criticism. We are the world's leader in agricultural research, and we invest very heavily through our land grant universities and virtually all of that research is published and anybody in the world can read it. But yet, since we did the research we have the practical experience. We train the graduate students. They are Americans. And then they go out and put it to work in producing a drought-resistant cotton; in producing an insect-resistant grain; and we benefit from it even though it is public research.

So I do not see that as a problem. I think that the plain truth is that you cannot keep technology a secret, and the way you stay the world leader is not by hiding it, but by moving ahead so that by the time your competition catches up with where you were, you have gone onto something else.

Let me also say, Mr. Chairman, I think part of the dispute in the scientific community is a very natural dispute, and the natural dispute basically is this: if I have gone into chemistry and I am working on DNA, I believe that that is the most important area in the world because in fact I have dedicated my life to it, and so I believe government money ought to go into that area.

Any time you are looking at a research project that is this big, you are always going to run into some of this rivalry within the scientific community. My own belief is that research creates tremendous spin-offs, and that this project is so big and so important that it is going to affect chemistry, that it is going to affect other areas, and the problem with trying to sell research politically is that you can never look into the future.

As Joe mentioned, much of the physics research done right at the turn of the century has produced products that now are so fundamental to our daily lives that we hardly even think about them having been generated only in this century. So there obviously is an element of faith in research and commitment to the future in these kinds of investments, and I realize that for the committee this is not an easy task, but I believe that the investments we made in research whether at land grant universities or whether in the space program or in other areas where we have invested in science and technology, I think that our record is a record of achievement and a record of great benefit to a large number of people from that research.

Mr. **Barton**. Mr. Chairman, if I could give you a practical application, obviously the research is going to be shared, but the technology, the actual construction of the components, if we locate the SSC in this country the facilities that build those components are probably going to be located not directly at the site, but somewhere domestically within the continental United States, and that has a job-creation benefit and a commercial technology benefit that is going to be near where the site is located. Also, the fact that we have the critical mass of the leading high-energy physics researchers in the world in the United States as they develop these concepts, the companies that take advantage of those concepts are going to want to be located near where the SSC is. It would not make sense to have 500 of the high-energy physicists working in the United States and yet the companies that take advantage of it locate their plant in the Ural Mountains in Russia, or wherever, so there are some practical benefits that are going to accrue to our country in locating and building the project in our country.

Senator **Ford**. Well, let me give you some practical experience. I think I have heard enough around here that we do the research and we never put it into the marketplace, and you can just name product after product, and the favorite one is the VCR or the television. Do you know one made in this country? No VCRs, and we discovered all of it. So it has all gone some place else.

Let us talk about the Defense Department and energy arena. In the 1970s we had 100 percent of enriched uranium business in the world, and now we have less than 40 percent. Last April, our domestic users, about 30 percent of them notified the government that they would no longer want to carry out their contract.

There was a lead time, and so about 30 percent of them gave notice last April, and we can expect an additional group this April, and the Russians have 2.5 million available to come into this country and now we are trying to work that out where they come in, and when that was brought to Secretary Herrington's attention, and now to new Secretary Watkins, they are very concerned about that.

We have the ALVIS, or the AVLIS—it is just a Yellow Creek expression. You see, I am bilingual. [Laughter.]

Senator **Gramm**. I speak your dialect, Mr. Chairman.

Senator **Ford**. But we were five years ahead of everybody else as it related to research in that field to do a better job in enriching uranium, but we have stopped. We have had to start sharing.

We have gotten into a fight over where the money should go and what piece it should go into, and now we will be lucky if we are on board first. And so where we were five years ahead and we have an opportunity to reduce our cost, we used the funding and charged it up to the so-called atomic energy, or the uranium enrichment arena.

We charged a lot of—the breeder-reactor that Senator Bumpers and I were not entirely in favor of, that cost was assigned there and that has put a heavy burden on it. So we talk about—the fight within the community is not going to be against a Superconductor, it is going to be against all of these we have had on board and we were working forward to complete and all of a sudden we find a new one on the horizon, and it is going to take all of the money or reduce the ability to keep the research going in other communities.

We have got a pretty tough row to hoe, as we would say at home, to try to figure out what to do. And it is not just this one, and if we go ahead and put the kind of money that we think is needed into the Superconductor, then we are going to be reducing others because we are not going to have the money. We are not going to find new money.

It is just not going to happen, and so when you start saying there is no new money, and I hear there is $87 billion that is out there and all of that but it is already gone, and yet you cut 88 projects completely out, or programs. It is going to be very tough. Senator Burns, I did not mean to take up so much time.

Senator **Gramm**. Mr. Chairman, could I just respond very briefly. I agree with you that we as a Nation have done a very poor job in translating our research into practical products, and I think it is an area that we need to do a lot more work in. I think we are beginning to make progress in doing that.

Some of our problems, like in uranium enrichment, we had a public policy dispute that impeded our ability, but the point is, if we do not do the primary research, if we do not have the lead, then we are never going to have an opportunity to move to development, and the fact that we have not used our opportunities in the past I think does not mean that we ought not to try to use them in the future.

Senator **Ford**. But Phil, that is what I am trying to say. We are doing it right now. We have got a new one on board and then we want money for that, and then we start nickeling and diming just barely keep it going and not really accelerate the research that we need, and we find out where that knowledge is available to others that will take it and go on with it, and so we find ourselves strained for funding.

We try to fund all these new things and all of a sudden we find ourselves nickeling and diming some of the research and we do not complete anything very well, and just kind of keep it on a level playing field. That is what bothers me.

I understand that this is supposed to be a great project and we will never see anything directly from it, it is the spin-offs that will come from this type of research.

I have been to the place and looked at it, and still do not understand it, but they tell me it is good. [Laughter.]

Senator **Burns**. It is one big race track.

Senator **Ford**. That would go good in Kentucky. We do not have any horses that could last four miles, or 53 either.

Senator **Burns**. Well, Mr. Chairman, I just have one question, I guess. Senator Gramm, we were kind of in on this thing too, you know, when the SSC was first proposed, and everybody got in this little war about where this facility was going to go, and just looking at the thing there is a smaller version of a collider that is in Europe, and to your knowledge, what is the difference? Is the SSC just one step beyond the research that is gained from that facility? I guess I should direct my questions to maybe the people in the scientific field.

Senator **Gramm**. Well, I can, I think, give you a basic answer, and the basic answer is this is a quantum jump beyond that. In essence, the best analogy to something that we are familiar with is a microscope, and what this will allow scientists to do is to understand the nature of matter and the composition of matter better and give us much greater insight than we have ever had before in understanding the mysteries of nature and therefore harnessing those mysteries to deal with the problems that we face.

Mr. **Barton**. To put it in numbers, the largest particle accelerator operating today is the Fermilab up in Illinois, and in its ring, each ring is one trillion electron volts. That gives you 2 trillion volts of collision energy.

This ring will be 20 trillion electron volts in each particle beam, and so that is 40 trillion, and so you are going from 2 trillion to 40 trillion, and you can look at much more—you look at the subparts of the proton, the very—according to the people behind me that can answer this a lot better, this lets them look as far as they think they need to look to find out the ultimate answer to the Einstein equation E=MC squared, where E is energy, M is the mass, C is the speed of light squared.

Ask Dr. Schwitters or Dr. Lederman if they testify. They can explain. But basically, we are going from two to 40. Now, I can understand that and I am an Aggie, so that is what we are trying to get at.

Senator **Gramm**. Take that and my microscope and you have probably got as good an answer as you are going to get.

Senator **Ford**. Are you an Aggie too?

Senator **Gramm**. I tried to teach Joe, but I did not teach him everything I know. Just everything he knows. [Laughter.]

Mr. **Barton**. Since I am in the Senate, I will not respond to that, Senator.

Senator **Ford**. Have at it. We love it. [Laughter.]

Senator **Ford**. Senator Bumpers.

Senator **Bumpers**. Mr. Chairman, I do not have anything to add. I cannot believe we are having two Aggies here tell us about the super collider. [Laughter.]

Senator **Ford**. And one is a teacher. [Laughter.]

Senator **Bumpers**. If I were from Texas I would be seated right where Senator Gramm and Congressman Barton are, and if it were being built in Arkansas I would be a lot less objective about the desirability and the fiscal ability to get this project done. Happily, since it is not being built in my state I can be reasonably objective about it and I must say that you make very good arguments, and I am deeply concerned about this.

You know, the super collider may be like the catalytic converter. It was not too difficult to build a converter that would take out 95 percent of the exhaust gases, nitrous oxides and so on, but the last 5 percent is prohibitively expensive, and so it may be with this.

But I just—I think about two or three things. The slurping up of so much basic research money, particularly in high-energy physics. The amount of money that you could start other basic—you could start 10,000 other basic research projects for what this one will cost.

It is just like, one of my real concerns about defense spending is not because I think it is bloated but because—and I have been told that 25 percent of the scientists in this country are involved in weapons development, whereas the Japanese keep about 1 to 2 percent of their scientists involved in defense and therefore we are at a distinct disadvantage.

Secondly, I do not understand why the Russians, who have devoted so much of their resources to high-energy physics are still a backward country. The Japanese have done virtually nothing in it, and they are eating everybody's lunch. So I am not at all sure we gain any technological or competitive advantage with a super collider.

Finally, if we are going to do it, it seems to me that this is the most ideal project in the world for international cooperation. I do not know why, for example, OECD countries could not put up a billion or a billion and a half, we put up a billion, or a billion and a half, the Japanese.

If it is really that desirable and the international scientific community agrees with that, I do not know why we have to foot all of the bill and I do not mind telling you, Senator Gramm and certainly you as the author of the Gramm-Rudman bill——

Senator **Gramm**. Do not forget Hollings.

Senator **Ford**. You dropped him there for a while.

Senator **Gramm**. The media liked him better than they liked Rudman and me.

Senator **Bumpers**. Warren Rudman says he has to carry hyphens around in his pocket to keep those three names straight. But I do not know why everybody cannot cooperate. I do not know where the money is coming from. I think it is a slurping up of a tremendous amount of money that could be spent for other items.

The governor of my state just got through giving $1 million of the Exxon overcharge money that came to our state to a physicist at the University of Arkansas who is one of the top two or three scientists in the country on superconductivity, Dr. Alan Herman, who led in the conductivity of electricity at the highest temperature for a while. Now it just keeps switching back and forth.

But when I went to see Dr. Herman and saw the kind of facility that he is doing one of the most important pieces of research in the Nation in, I was just appalled and so was the governor.

But I could not get him $1 million, and I sit on the Appropriations Committee and I could not get him $1 million, and if he happens to be successful in this we are going to save 15 percent of all the electricity we generate in this country, and he is one of the top two or three and I cannot get him $1 million.

The governor had to give him $1 million out of his Exxon overcharge money, and yet we are talking about $5 billion here for a project that will take so much of our resources.

But those are just my general thoughts about this. I am not unalterably opposed to this, but I think we ought at least search for alternative methods. I think that perhaps I had a Nobel physicist in my office not long ago who is strong for this project as designed and who thinks that the consortium talking about building one, the one in Switzerland, is just not adequate for a great Nation like the United States, that we ought not to join that consortium. And I do not know.

But that is the reason I am here today. I want to hear—and honestly, I know nothing about physics. If any of my children had been able to add three and three I would have known Betty had been messing around, because we do not have any scientific acumen in our household, and I never have. I have not even been curious about it.

But I am curious as to whether or not this is the best use of our resources. I want us to be the leader of the world in high-energy physics if it is practical to do it, and if we get enough spin-off from it.

Senator **Gramm**. Senator, could I respond to that? First of all, let me say that I cannot answer every question you raise, but let me try to give you some reasons why I believe that the level of participation we get abroad, while I believe we will get some, I do not think it is going to be massive as compared to the overall cost of the project even though I think that once we get the full, once it is clear we are going ahead, that we may get the $500 million, or billion dollars from abroad.

I think the reason that we are not going to see a larger investment is that foreign nations will understand that the spin-offs are going to tend to be in geometric, in geographic symmetry to the site itself; that what it means is that their best scientists are going to be coming here. They are going to be working here. The companies that will develop the spin-offs, that will be using them as a knowledge source, will tend to be in the United States and not in Europe and not in Japan. That having the project here will create the scientific mass in our country, not in Europe and not in Japan.

So they will benefit in terms of pure science but in terms of the technological spin-off, the job creation, that will occur directly attributable to it, we are going to be the first beneficiary because the site itself and the people who will run it and the people that will be trained by using it will be here.

So it is the same reason that Joe and I are here and Senator DeConcini and Senator Domenici are not here. We are here because this is in our state, and while it is a big project and will spill all over the country, obviously a lot of it is going to be in the Southwest, and I think that that is the explanation of why other countries that are willing to contribute to get access to send their scientists, my thesis is that they believe that we are going to be the great beneficiary by having it here and that, in a sense, while it helps them in their scientific endeavors it also creates a brain-drain on them, and we benefit from the technology.

Mr. **Barton**. Senator, if I could just give you a few numbers that your wife and your family would understand—and I have a feeling you understand a little bit more than you let on. I might point out that an Arkansas businessman is alleged to be purchasing the Dallas Cowboys today, so you may become a Cowboy fan if that occurs, and you may be spending——

Senator **Bumpers**. I have always been a Cowboys fan.

Mr. **Barton**. Currently, the high-energy physics budget I think is around 7 percent of our basic science budget. In the peak year of construction for the SSC it would go up to about 12 percent of the basic science budget, and that is during construction, and then it would go back down once we build the project, so even in the peak year this crowding out analogy just does not bear water. We are not going to go to 60 or 70 percent of the basic science budget of this country. So that is the first point I would make.

The second point I would make is, let us assume we do not build the SSC. It does not automatically follow that there will be more money funded for little science. You need some big science projects that generate enthusiasm in the public perception, just like the space program in the 60s.

Obviously a lot of that money went for the NASA Apollo program, but it generated excitement and more people got into science because of that, and there was a tremendous synergistic effect. The SSC would have the same effect, in my opinion, in the late 90s and early 2000s.

Senator **Bumpers**. That is a very plausible argument, Congressman.

Senator **Ford**. We have been honored by the presence of Senator Bentsen from the great State of Texas and senator, we have listened to your colleagues on your right. We have been asking some questions of them as a result of their statement. If you would like to proceed with your statement we would be delighted to hear you at this time.

STATEMENT OF HON. LLOYD BENTSEN, U.S. SENATOR FROM TEXAS

Senator **Bentsen**. Thank you very much, Mr. Chairman. I appreciate that and I apologize for the fact that I did not have any better commitments, I just had earlier commitments, and I was not able to get here.

Senator **Ford**. Well, that gets you a lot of places. [Laughter.]

Senator **Bentsen**. I am sure it will not come as any surprise to you that I am a very strong supporter of the Superconducting Super Collider.

Senator **Bumpers**. Could you pull the microphone a little closer please, Lloyd?

Senator **Bentsen**. I apologize for the rasp in my throat this morning.

I am here as a very strong supporter of the $250 million that was in the Reagan budget and now in the Bush budget. I think it is imperative that we have it. In this day of budgetary constraints you are seeing a reaching out for additional funds to try to get some assistance in trying to build this very major project, the State of Texas committing itself to $1 billion in that regard, and that is for a state that has really taken a heavy economic hit, and so it is a very major contribution and substantial savings for the rest of the taxpayers around the country.

I believe that we are seeing here a long-term scientific project that will help restore the United States leadership in particle physics, and I believe any substantial shortfall in that funding would be very damaging to the project.

It is not only a unique project for scientific research, but it also is a new international basic research laboratory, attracting the world's best scientists to work on expanding the frontiers of human knowledge and understanding. I believe it is a well-planned undertaking based on mature technology, and that is indispensable to timely progress of science.

We just cannot calculate all the technological, economic and scientific benefits that are likely to flow from SSC, but every time we have broken through frontier research we have seen the benefits that have accrued to us. We have seen it medical science, computer technology, electronics, various other high tech fields, where we have taken the lead on basic research.

What is happening to us in this country is a lessening in the amount of funds that are being spent for R&D as related to RG&P. What is happening to us is related to our international competition with Germany, with the Japanese, with a number of other leading economic forces around the world, and I think it is important that we proceed with it.

Mr. Chairman, you have heard my colleagues testify at length on this and answer questions, and I would like to put the balance of my statement in the record if I may.

Senator FORD. Without objection, it will be included in the record in its total.

[The prepared statement of Senator Bentsen follows:]

Statement by Senator Lloyd Bentsen

Senate Subcommittee on Energy Research

MR. CHAIRMAN, MEMBERS OF THE SUBCOMMITTEE, THIS HEARING IS ON A SUBJECT THAT IS IMPORTANT TO TEXAS BUT ALSO IMPORTANT TO OUR COUNTRY AS A WHOLE. I AM HERE TODAY TO URGE YOU TO APPROVE $250 MILLION TO BEGIN CONSTRUCTION OF THE SUPERCONDUCTING SUPER COLLIDER (SSC). THAT IS THE AMOUNT IN THE REAGEN BUDGET, THE BUSH BUDGET, AND IT'S WHAT THIS CRUCIAL PROJECT NEEDS. ANY SUBSTANTIAL SHORTFALL IN FUNDING COULD DELAY THIS PROJECT AND THE MANY SCIENTIFIC BENEFITS WHICH WE EXPECT TO DERIVE FROM IT.

AS YOU KNOW, THE SSC IS A LONG-TERM SCIENTIFIC PROJECT TO RESTORE U.S. LEADERSHIP IN PARTICLE PHYSICS. THE SSC WILL BE NOT ONLY A UNIQUE INSTRUMENT FOR SCIENTIFIC RESEARCH BUT ALSO A NEW INTERNATIONAL BASIC RESEARCH LABORATORY, ATTRACTING THE WORLD'S BEST SCIENTISTS TO WORK ON EXPANDING THE FRONTIERS OF HUMAN KNOWLEDGE AND UNDERSTANDING. THE SSC IS A WELL PLANNED UNDERTAKING, BASED ON MATURE TECHNOLOGY, THAT IS INDISPENSABLE TO THE TIMELY PROGRESS OF SCIENCE.

WE CANNOT CALCULATE ALL THE TECHNILOGICAL, ECONOMIC AND SCIENTIFIC BENEFITS LIKELY TO FLOW FROM THE SSC. HISTORY, HOWEVER, HAS REPEATEDLY SHOWN THE BENEFITS OF FRONTIER RESEARCH. THE BENEFITS OF PAST ACCELERATOR RESEARCH HAVE LED

TO ADVANCEMENTS OUTSIDE THE SCIENCE OF PHYSICS. MEDICAL SCIENCE, COMPUTER TECHNOLOGY, ELECTRONICS, AND VARIOUS OTHER HIGH-TECH FIELDS HAVE ALSO BUILT ON THAT BASIC RESEARCH. MILLIONS OF PEOPLE HAVE BENEFITTED FROM SUCH MEDICAL ADVANCES AS PROTON THERAPY FOR TREATMENT OF CANCER AND MAGNETIC RESONANCE IMAGING. IMPROVEMENTS IN SEMICONDUCTOR TECHNOLOGY USING ION IMPLANTATION HAVE ALLOWED AMERICAN COMPUTER, ELECTRONICS, AND COMMUNICATIONS INDUSTRIES TO REMAIN COMPETTITIVE IN THE WORLD MARKETPLACE.

SOME CRITICS FEAR THAT THE SSC PROJECT WILL BE "SQEEZING OUT" OTHER WORTHWHILE SCIENTIFIC RESEARCH PROJECTS. THAT NEED NOT OCCUR, AND IT SHOULD NOT OCCUR.

OUR COUNTRY MUST MAKE INVESTMENTS TODAY, PARTICULARLY IN SCIENCE, IF WE ARE TO GUARANTEE OUR FUTURE ABILITY TO COMPETE IN A WORLD OF RAPIDLY EVOLVING TECHNOLOGY. I WOULD ARGUE THAT WE SHOULD BE INCREASING OUR R&D EFFORTS IN ORDER TO REMAIN COMPETITIVE WITH OUR INTERNATIONAL TRADING PARTNERS. BUT, SOME FRONTIERS OF KNOWLEDGE CAN BE EXPLORED ONLY WITH COMPLEX AND COSTLY EQUIPMENT AND TEAMS OF SKILLED SCIENTISTS.

I AM ACUTELY AWARE OF THE NEED TO REDUCE THE FEDERAL DEFICIT AND RESTRAIN EXPENDITURES. THE SSC IS SO IMPORTANT IT IS ATTRACTING SUPPORT WHICH REDUCES THE BURDEN ON THE FEDERAL BUDGET. THE DEPARTMENT OF ENERGY IS SEEKING INTERNATIONAL PARTICIPATION AND COST SHARING. AND, THE STATE OF TEXAS HAS

COMMITTED ONE BILLION DOLLARS. THAT'S REAL MONEY - FOR A PROJECT TO BE BUILT IN OUR STATE BUT WITH BENEFITS AND SUPPORTING CONTRACTS ACROSS THE COUNTRY.

JUST AS WE MAKE ROOM IN THE BUDGET FOR CRITICAL NATIONAL DEFENSE PROGRAMS AND FOR MEETING BASIC HUMAN NEEDS, WE ALSO MUST PROVIDE FOR BASIC RESEARCH TO SECURE AMERICAN LEADERSHIP IN SCIENCE AND TECHNOLOGY. SUCH LEADERSHIP IS ESSENTIAL FOR OUR ECONOMIC GROWTH, OUR INDUSTRIAL COMPETITIVENESS, AND OUR NATIONAL SECURITY. THE SSC IS A KEY ELEMENT IN A COMPREHENSIVE PROGRAM FOR SCIENTIFIC EXCELLENCE.

I URGE YOU TO GIVE THIS PROJECT THE FUNDS NEEDED TO KEEP IT ON TRACK, ON SCHEDULE, AND INTO CONSTRUCTION DURING 1990.

Senator FORD. Senator Conrad, did you have any questions?

Senator CONRAD. I appreciate, Mr. Chairman, this opportunity. I would like to ask the witnesses, given the current fiscal status of the federal budget and the significant imbalance that exists there, would you believe that this project is sufficiently important that you would support a revenue increase in order to pay for this substantially increased expenditure?

Senator BENTSEN. Well, let me say, if I may, that is one no one wants to answer I suppose. Let me step up to that one. I think when we get all through with this budget you are going to see some of the most creative terminology on revenue enhancers we have ever seen. [Laughter.]

Senator BENTSEN. I can recall in the summit meeting, the economic summit meeting in December of 1987, people from the Treasury coming to me and saying can you just give us something that we can take back to the old man, that we can say is not a tax, that we can call it something else. I think we just began the process. I think you are going to see it happen this time, and I think you will see revenue enhancers in the final budget solution.

Senator GRAMM. Well, let me respond by saying that I believe if you look at the kind of investment that we are making in the next generation, that whether you decide on raising revenues or whether we decide not to raise revenues, that if we decide not to raise revenues we are talking about limiting spending to roughly $1,150,000,000,000; that I believe within that amount that there is room for investment in the future to create jobs and growth in technology and to keep America the number one generator of technology in the world.

So I think the answer is, this project makes sense no matter what you are going to do about taxes, because if you go down all of the programs we fund and the amount of money that is available with the tightest budget, which would be with no taxes, I believe there is room in that budget for $250 million invested in the future of science and medicine and defense and our fundamental understanding of the world we live in.

I think that is especially borne out when you look at the return we have had in the past in the investments that we have made in technology and in science, whether it has been through ag research or whether it has been high-energy physics.

Senator CONRAD. Let me, if I might Mr. Chairman, follow up. That is an artful duck.

Senator GRAMM. No, it is not an artful duck at all. My answer is, without a tax increase this project is justified. With a tax increase this project is justified.

Senator CONRAD. But that was not the question. Let me, if I might, try to get an answer to my question. I got a pretty clear answer from Senator Bentsen. What I would like to know from you is, look, this budget is far out of balance. You are here advocating a spending project. I would like to know, if it came down to it, and the decision was in the $1,150,000,000,000 budget that we have we are not going to include this unless there is additional revenue to pay for it, would you support additional revenue to pay for this project?

Senator **Gramm**. Let me respond by saying the decision is not going to come down to that. I mean, if you are asking me do I support a tax increase the answer is unequivocally, no. I do not.

Senator **Conrad**. Even if that would mean that this project were not——

Senator **Gramm**. No, that does not mean this project is not funded. The point I am making is that whether you raise taxes or whether you do not raise taxes, I believe when you compare this project to the return that America gets from Amtrak, when you compare this project to the return that this Nation gets from the Appalachian regional development program, when you compare the return that the Nation gets from UDAG, I am saying that the decision on taxes is going to be made at some point and there is going to be a debate, and that a decision is going to be made and we may or may not raise taxes.

But my point is that the support for this program should not be contingent on that, because I do not view this as a marginal program. This is not—and I do not view the increase for NSF as a marginal program and I do not view the space station as a marginal program. Those are programs that are yielding a lot greater return to the future of America than a lot of programs that will be funded whether we raise taxes or whether we do not.

Senator **Conrad**. Well, you know, that is part of the legislative process. You have your set of priorities and other members have their set of priorities. The problem we have got, from my observation, we have got a whole lot of people willing to spend money and not a sufficient number willing to finance that spending.

It is real easy to come in here and be in favor of all the spending projects and not be willing to finance the spending, and that is what I see happening across all of Congress, and now we have you, one of the foremost advocates of being tough on spending and you are in here promoting a spending project which would benefit your constituents, just as the rest of us, you know, advocate projects that will benefit our constituents.

Senator **Gramm**. Well, if I may respond. Let me remind you, the purpose of Gramm-Rudman is not to eliminate the government but to balance the budget, and within a balanced budget constraint we can spend $1,150,000,000,000 next year. I am here to say that whether we raise taxes or whether we do not raise taxes, we ought to be investing in programs that affect the next generation and not spending so much money on programs that affect the next election.

Senator **Bumpers**. If the senator would yield, I would just like to make a comment on that. If we spend, senator, $1,150,000,000,000 next year, we will still have a structural deficit of $240 to $250 billion.

Senator **Conrad**. If I might, I have got the numbers right here. We will have a structural deficit of $263.5 billion. Because the fact is, the fact is, as we are so-called reducing the federal budget deficit, that we officially announced the trust funds that are being raided in order to finance this are increasing, and increasing dramatically.

In fact it will be $148.5 billion that we will be spending more than we take in because we are looting the trust funds. So I do not think—frankly, Gramm-Rudman is not balancing the budget unfortunately. If you look across the years—and I asked my staff for the chart yesterday—the last 12 years we are going with dramatic increases in what we are taking out of the trust funds, and all of what we see in terms of reduction on the official deficit is being offset by the increase in what we raid from the trust funds.

If it came down to this senator deciding, I am not going to support this unless I see senators who benefit being willing to stand up and support revenue, your answer to me would be, I will not support revenue.

Senator GRAMM. My answer to you is that in the budget that we have to write to conform with the Balanced Budget and Emergency Deficit Control Act, that there is room for this project and there are many projects that would in any kind of logical priority setting be thrown overboard before this project.

So my response is, I do not see your question as being relevant to the decision about this project.

Senator CONRAD. Well, it may be relevant to my decision, maybe not to yours. What I am saying to you is, in my list of priorities, this is not very high. It is high in your list because this benefits your state. Other things benefit my state, which you are quick to oppose, and I am saying to you, if I do not see you willing to stand up and say you are going to support revenue for your project, I may well not be for it.

Senator GRAMM. Well, you can make the choice as you want. My point is, I believe that this project is a high-priority item.

Senator FORD. Gentlemen, my wife always lets me get in the last word: Yes, ma'am. [Laughter.]

Senator FORD. So we are going to be able to debate the question on financing and budgeting and so forth. We are here today to look at the project. The Senator from North Dakota brings up a good point, and I have a few little things I am interested in that come through this agency too, and—through this committee—and I understand where he is coming from, but we need to get some of those behind you that are for and against this also, who come from the scientific field, and I have never enjoyed anything more in my life in the year 1989 than this little question and answer period here. But we have been going for an hour and I think that is long enough, and we will get to it, and I do thank you, senator.

Senator BENTSEN: Senator Ford, could I just make one comment here on this?

Senator FORD. I am going to be my wife then, senator. [Laughter.]

Senator BENTSEN. I would say, Mr. Chairman, when the distinguished Senator from North Dakota talks about benefiting members, he is right. It does benefit us in Texas. But it goes far beyond that.

We are talking about research and development that benefits our Nation, and what we have seen happening to us as a percentage of our R&D, it has tilted substantially toward the military, and we are not getting the spin-offs from that R&D that we were getting at some times in the past, so your civilian R&D is a lessening amount as compared to other major competitors of ours in international trade.

That is, along with the budget deficit we also have this trade deficit, and it is terribly important that we do the kind of research that will help us meet that kind of competition, and I think that is where this makes a long-term overall resource for our country and not just pay-off for the State of Texas.

Senator **Ford**. Thank you, gentlemen. We appreciate it, and you can rest assured this will be debated further.

In order to move on and try to get out of here at a reasonable hour this morning, I am going to ask the next three witnesses if they will just all move to the table together.

That would be Dr. Hunter, Dr. Schwitters and Dr. Anderson. If all three of you will come to the table, we will hear your testimony and hope that you can highlight it and then give the senators an opportunity to ask some questions.

Dr. Hunter, if you want to proceed, we would be delighted to hear you, and then we will go to—and if you could summarize Dr. Hunter, and we will ask the others to do the same, we will go to Dr. Schwitters from there and then to Dr. Anderson. Dr. Hunter.

STATEMENT OF DR. ROBERT O. HUNTER, DIRECTOR, OFFICE OF ENERGY RESEARCH, DEPARTMENT OF ENERGY

Dr. **Hunter**. Thank you, Mr. Chairman. Mr. Chairman and members of the committee, it is my pleasure to appear before this committee to discuss the Department of Energy's budget request for the fiscal year 1990 basic research programs. I have submitted written testimony for the record, and with your permission I will proceed with some brief summary comments.

Senator **Ford**. Your statement will be included in the record in full, Dr. Hunter.

Dr. **Hunter**. Thank you, sir. I am here today to discuss several areas that are included in the General Science and Research and Energy Supply Research and Development accounts. The topical areas, include High Energy Physics, Nuclear Physics, Basic Energy Sciences, Biological and Environmental Research, educational programs and university support. Before we go into the details of the different areas, I would like to point out some elements common to the conduct of research in these areas.

First, the great bulk of the work is basic research. In basic research, competition among the different researchers leads to utilizing the best means in a given area to obtain the information needed.

Second, the urgency for basic research is not easy to establish, since the consequences of the information are not explicitly known. We rely on the observation that to produce the best scientists and scientific results, you must work in a fashion that forces the pace of advance in an area.

Third, it follows that the scientific communities, through a variety of mechanisms for conducting scientific competition, such as advisory review committees, are the basic means for determining the direction and form of the research that is to be pursued.

In accordance with these points, to enable these communities to pursue research and assure the highest quality of basic research, the Department of Energy relies on the best scientific talent to generate its research programs. This year, for example, the Nobel Prize in Physics was granted to Drs. Lederman, Schwarz and Steinberger for research done on facilities built and supported by this program. Dr. Lederman has been the head of Fermilab, one of our major facilities, for many years.

Also, as a part of this we must continue to invest in new facilities people and ideas. These efforts that are conducted to build new tools, both experimental and theoretical, are part of this process. The efforts conducted under this program are intended to provide scientific excellence by continuing to utilize the existing facilities and, critically, to provide new facilities to best pursue the frontier research in the different areas, no matter what the size.

These programs are of benefit to widespread scientific communities. In today's science, modern communications and transportation have made it possible to build common state-of-the-art tools that are used by very many parties. In the programs under discussion, we must not lose sight of the fact that even the largest tools are used by very many small groups, universities and laboratories dispersed on a national, and indeed an international, scale.

In this fashion, federal support of science supports and encourages the diversity of expression and competitive science for very many parties. This office, Energy Research, uniquely supports the research itself, the development of facilities, and the support of the facilities for many parties. We have to ensure the optimum utilization of our resources. Obviously, the development and operation of new tools is a major task and must be undertaken continuously.

This year, we are building and starting several new large instruments, all of which serve large laboratory and university communities. These include the SSC, forwarded as the major next step for the high-energy physics community, a step of enormous potential and significance; and the advanced photon source, known as the 6-7 GeV Synchrotron Radiation Source, for generating precise high-energy X-rays for 50 to 100 user groups simultaneously. This is to be started this year. An earlier generation 1/2 GeV Synchrotron Radiation Source X-ray machine is well under construction at Lawrence Berkeley Laboratory.

We are continuing major construction for CEBAF for the nuclear physics community, and there are several others as well, so you see that our instrument development supports the many communities and areas sponsored under the auspices of these programs.

There are many on-going races at the moment, but let me mention two: one in X-ray lithography for semiconductor fabrication, as developed on a facility at Brookhaven National Laboratory by IBM, and one in high-energy physics, where the Stanford Linear Collider is just coming on line at Stanford.

We have to ensure adequate support by the standards outlined for university and laboratory groups across the entire spectrum of basic research. This committee has long been a major and responsible supporter of such research. In the face of the value of basic research and in the present fiscal environment, the Administration shares the determination of the committee to see substantial and real growth in basic research. We believe this is the way to invest in the future.

We look forward to working with you to assure the future of basic research, and to assure the continuation and expansion of these areas of research.

I would be happy to answer any questions.

[The prepared statement of Dr. Hunter follows:]

Statement of Robert O. Hunter

Director

Office of Energy Research

Department of Energy

before the

Subcommittee on Energy Research and Development

of the

Senate Committee on Energy and Natural Resources

February 24, 1989

Mr. Chairman and Members of the Subcommittee:

It is a pleasure to appear here today to present the FY 1990 budget request for several of the programs supported by the Office of Energy Research (ER). These programs are: High Energy Physics, the Superconducting Super Collider, Nuclear Physics, Basic Energy Sciences, Biological and Environmental Research, Energy Research Analyses, University Research Support, University Research Instrumentation, Multiprogram Energy Laboratories-Facilities Support, and supporting program direction.

Funding for these programs in FY 1990 is being requested under the two appropriations shown in Table 1, Energy Supply R&D and General Science and Research. The total FY 1990 budget request for ER's programs is $2.5 billion, an increase of $292 million over the FY 1989 appropriation of $2.2 billion. This request reflects the Department's recognition of the key role played by science in enhancing the economic and technological well-being of the United States, as well as the quality of our lives. It also reflects our continuing commitment to excellence in the programs supported by the Office of Energy Research. I would like to discuss the programs under the General Science and Research appropriation first, followed by those under the Energy Supply R&D appropriation.

HIGH ENERGY PHYSICS

Research in high energy physics pushes beyond the existing frontiers of knowledge about the physical world in which we live. This research not only helps us to learn how our world works, it leads to the development of advanced

Table 1
Office of Energy Research
Summary by Program
Budget Authority
($ in millions)

	FY 1988	FY 1989	FY 1990 Request
Energy Supply R&D			
Basic Energy Sciences	$ 557.7	$ 549.3	$ 590.0
Magnetic Fusion Energy	331.3	350.7	349.2
Biological & Environmental Research	267.1	257.5	271.4
University Research Support	18.2	21.7	16.1
University Research Instrumentation	4.9	5.0	5.0
Energy Research Analyses	3.7	3.8	4.0
Advisory & Oversight Program Direction	3.2	3.3	3.4
Policy and Management	.7	.7	.7
Multiprogram Energy Laboratories-Facilities Support	53.6	55.7	53.0
Small Business Innovation Research	30.1	0	0
Subtotal	$ 1,270.5	$ 1,247.7	$ 1,292.8
General Science & Research			
High Energy Physics	$ 526.1	559.4	616.2
Superconducting Super Collider	33.0	98.6	250.0
Nuclear Physics	242.8	260.9	299.3
General Science Program Direction	2.8	3.2	3.9
Transfer of Prior Year Unobligated Balances	(8.0)		
Subtotal	$ 796.7	$ 922.1	$ 1,169.4
Total ER	$ 2,067.2	$ 2,169.8	$ 2,462.2

technology needed to ensure research progress--technology which, over the years, has had many practical applications in such areas as nuclear medicine, electronics, computer chips, superconductivity, and cryogenics.

The continuing goal of the High Energy Physics program is to improve our understanding of the nature of matter and energy and of the basic forces that govern all processes in nature. High energy physicists search for new knowledge through an interplay of experimental research using accelerators and detectors, and through theoretical studies. The ability to carry out forefront exploratory research on the physics frontier is dependent on many factors including the experimental capabilities of accelerator, colliding beam, and detector facilities; the effective use of those facilities, and the incorporation of upgraded and new facilities on a timely basis to take advantage of new technologies and research opportunities.

Progress in the experimental and theoretical research supported by the High Energy Physics program is also dependent on scientists from over 100 universities who carry out over three-fourths of this research. They provide much of the intellectual leadership for the field along with the scientists from the national laboratories who make up the rest of the experimenters. The High Energy Physics facilities also play an important role in the training of highly skilled scientists and engineers who often contribute significantly to the transfer of technology to other fields.

Our understanding of the nature of matter and energy and the basic forces has grown substantially in recent years; but the search continues for a unified, or single, theory that can explain the connections among the basic forces of nature. The four known fundamental forces are the electromagnetic force, the weak force, the strong force, and the gravitational force. A part of the progress in high energy physics has been the conceptual unification of the weak nuclear force and the electromagnetic force into a single "electroweak" framework. In addition, a theory of the strong force has emerged which is consistent with experimental results. There is increasing anticipation that the unification of the strong nuclear force and the electroweak force into one "grand unified" framework may be possible, thus reducing the apparent number of forces in nature to two. The ultimate goal is a "super-grand" unification that would bring the fourth force, gravity, into the equation, thus bringing all of the forces in nature into a single unified framework.

Based on this progress, scientists have drawn a picture of nature known as the Standard Model which is defined in terms of the electroweak and strong forces and a small set of fundamental constituents present in all matter we see around us. Although the most significant recent experimental results all reinforce the predictions of the Standard Model, scientists believe that it cannot be the final answer. For example, it does not explain the origin of mass and it does not include gravity. Clearly, we do not yet know the final answer to many questions. For instance, have we discovered the ultimate constituents of matter or is there something inside leptons and quarks, which we believe are the fundamental particles. Do we know all the forces of nature and are all of the forces a manifestation of a single force?

Research aimed at answering these questions is currently being carried out at accelerators at three centers supported by the Department of Energy: the Fermi National Accelerator Laboratory (Fermilab), the Stanford Linear

Accelerator Center (SLAC), and the Alternating Gradient Synchrotron (AGS) at Brookhaven National Laboratory (BNL). Each of these laboratories provides world unique capabilities and is operated as a national facility available to qualified experimenters from around the world on the basis of the scientific merit of their proposals. This practice of relying on one-of-a-kind, large, advanced facilities has a long track record of success. The talented physicists attracted by research opportunities at these facilities have continued to produce impressive, Nobel-class research results. Of the two new major U.S. High Energy Physics facilities, the Fermilab Tevatron Collider is now in operation for research; and the Stanford Linear Collider (SLC) has successfully completed a period of accelerator research and is now in the final stages of preparation for physics research. These facilities will keep the U.S. program at the cutting edge for the next several years. To maintain an internationally competitive program at the scientific frontier in the late 1990's, the Superconducting Super Collider (SSC) is being proposed for initial construction in FY 1990. The SSC, which will be discussed later in this statement, is in keeping with the successful High Energy Physics tradition of large, advanced accelerators operated as user facilities that attract the best talent and produce the most promising and exciting research results.

Before turning to a discussion of the FY 1990 budget request, I would like to summarize some of the progress made at the three High Energy Physics accelerator facilities. The Tevatron at Fermilab has been running at world record energy and luminosity in the colliding beam physics run which began in late FY 1988 and will continue into mid-FY 1989. The higher luminosity available at the Tevatron, which exceeds its design by a factor of two, allows scientists to observe a larger number of particle interactions of interest than is possible at the other facilities. During the Tevatron physics run the Collider Detector at Fermilab (CDF) is intensively searching for the as yet unobserved particle known as the top quark. The existence of the top quark has been predicted by the Standard Model. Thus, the top quark search--which puts Fermilab and the European high energy facility, CERN, in a vigorous race for a major discovery--provides a crucial test for this fundamental theory. In fixed-target experimentation at Fermilab, an 8 1/2-month long run was successfully completed in 1988. Eight major experiments were completed; data analysis is in process and physics results will begin to emerge during 1989.

At SLAC, the Stanford Linear Collider (SLC) has reached its first goal of demonstrating that two very small (a fraction of the diameter of a human hair), very high energy beams of electrons and their oppositely charged cousins, positrons, can be brought into head-on collision for a number of hours. The SLC has demonstrated the concept of linear colliders using fifty billion electron volt (GeV) beams and is providing crucial data on the feasibility of the next generation higher energy linear colliders. This concept will likely be the foundation for the next generation of electron-positron colliders. The second goal of the SLC is to study the Z^0 particle, which is the carrier of the weak nuclear force. Commissioning of the SLC to study Z^0 physics is underway, and the SLC is expected to produce several thousand Z^0 particles for study in FY 1989. Detailed study of the Z^0 particle, of which only a few hundred have been observed in the world to date, provides yet another key test of the Standard Model and will shed light on the possible existence of a whole new generation of particles. Also at SLAC, the Time Projection Chamber detector at the Positron-Electron Project (PEP) has just completed its first physics run since the completion of detector and beamline upgrades. The previous record for luminosity has been doubled. Data are now being analyzed with emphasis on measurement of decay lifetimes of short-lived particles.

At BNL, experiments are underway at the Alternating Gradient Synchrotron to study certain rare ways a particle called the kaon decays. The kaon can decay into many different combinations of other lighter particles. Some of these combinations occur very rarely, less than once in three billion ordinary decays. Comparison of experimental results with theoretical predictions will test current theories. The current experiments have already achieved results 10,000 times more sensitive than previous experiments and they offer the opportunity to find experimental evidence for new particles of very high mass. In addition, the AGS multiparticle spectrometer has the best evidence for the experimental identification of an unusual and, as of yet, unseen but predicted particle called a glueball. The quarks inside of protons, for example, are believed to be held together by exchanging an object called a gluon. Theory predicts that if two gluons combine, they would form a glueball. Observation of such objects would provide a new test of the Standard Model.

FY 1990 HIGH ENERGY PHYSICS BUDGET REQUEST

The FY 1990 request for High Energy Physics is $616.2 million. Of the total amount, $492.7 million is for Operating Expenses, $85.5 million is for Capital Equipment, and $38.0 million is for Construction (Tables 2 and 3).

Table 2
General Science and Research
High Energy Physics
Budget Authority
($ in millions)

	FY 1988	FY 1989	FY 1990 Request
Operating Expenses			
Physics Research	$114.4	$121.7	$133.1
Facility Operations	230.1	252.4	277.6
High Energy Technology	63.7	65.1	82.0
Subtotal	$408.2	$439.2	$492.7
Capital Equipment	76.7	79.8	85.5
Construction	41.2	40.4	38.0
Total High Energy Physics	$526.1	$559.4	$616.2

Table 3
General Science and Research
High Energy Physics
Budget Authority
($ in millions)

Construction

Project	TEC	FY 1989	FY 1990 Request
Accelerator Improvements and Modifications (90-R-101)	$14.9	$ -	$ 14.9
Accelerator Improvements and Modifications (89-E-101)	11.7	11.7	-
General Plant Projects (GP-E-103)	13.4	-	13.4
General Plant Projects (GP-E-103)	11.1	11.1	-
AGS Accumulator/Booster (86-R-105)	31.7	14.0	5.0
Fermilab Linac Upgrade (90-R-104)	22.8	-	4.7
Fermilab Central Computing Facility Upgrade (86-R-104)	24.6	3.6	-
Total Construction		$ 40.4	$ 38.0

The Operating Expenses request of $492.7 million reflects strong utilization of existing forefront facilities, particularly the Tevatron proton-antiproton collider and the SLC electron-positron collider. In FY 1990, advanced high energy technology R&D will focus on increasing the luminosity of both the Tevatron and the SLC to maximize the physics output of the existing and new detectors.

The Operating Expenses request also provides for operation at SLAC of the recently upgraded PEP with the improved Time Projection Chamber detector as well as operation at BNL of the Alternating Gradient Synchrotron (AGS) program. The AGS program focuses on rare kaon decay, neutrino and unique polarized proton experiments. Leon Lederman, Director of Fermilab, along with Mel Schwartz and Jack Steinberger, won the 1988 Nobel Prize in Physics for early work on the still very productive AGS.

In keeping with the planned strong use of existing facilities, the Operating Expenses request includes increases for university-based scientists to allow their effective participation in research at these facilities. Funds are also being requested for improvement of the technical capabilities of the university laboratories. The universities play a major role in physics research, detector R&D, and detector fabrication. The requested funds will provide for upgraded instrumentation (e.g., computer-aided design systems, high-speed electronic test equipment, etc.) and broadening and upgrading of the technical support staff at universities.

The FY 1990 Operating Expense request includes funding to support an increased level of technology R&D in support of the operating accelerators, primarily the SLC and the Tevatron. This R&D is crucial in identifying and providing for the modification and improvements needed to maintain the capabilities of the facilities at the cutting edge of the science.

Finally, the FY 1990 Operating Expenses request includes funds to carry out advanced accelerator and detector R&D to develop advanced concepts and technologies. Test-bed verification of some of the most promising of these concepts will be emphasized in FY 1990.

The FY 1990 Capital Equipment request of $85.5 million will provide the particle beam-line components, detection apparatus, and data analysis systems essential to forefront high energy physics research. In FY 1990, priority will be given to completion of the Stanford Large Detector at SLAC and the D-Zero detector at Fermilab. The request also provides for an upgrade of the equipment at the SLAC Central Computing Facility.

The FY 1990 Construction request of $38.0 million includes funds to initiate the Fermilab Linac Upgrade project, a key element in maintaining a vital research program with the Fermilab Tevatron in the years before the SSC becomes operational. This $22.8 million project, with $4.7 million requested in FY 1990, will replace a portion of the accelerator hardware in the existing linac for which commercial replacement parts are no longer available. The upgrade will also result in an increase in the output energy from 200 to 400 million electron volts (MeV). This higher energy produces a smaller beam, better injection, and smaller beam losses in the booster, main ring and

Tevatron thereby increasing the beam intensity and physics output. The Construction request also includes funds for the completion of the Alternating Gradient Synchrotron Accumulator/Booster project at BNL and an increased level of effort for Accelerator Improvement Projects and General Plant Projects to support the accelerator facilities and essential environmental protection efforts.

SUPERCONDUCTING SUPER COLLIDER

The Stanford Linear Collider at SLAC and the Tevatron at Fermilab, which I discussed previously, will keep the U.S. High Energy Physics program at the cutting edge of physics research for the next several years. But even these facilities will not reach high enough energies to allow physicists to investigate the region where major breakthroughs are anticipated. To advance the research frontier a new, more powerful, accelerator is needed. The Superconducting Super Collider (SSC) is that facility.

The SSC will consist of two rings of superconducting magnets and associated systems in a common tunnel, 53 miles in circumference. There will be four interaction regions where two beams of protons will collide head-on, making available a total of 40 trillion electron volts (TeV) of energy. These energies are expected to produce new types of matter and new forms of energy. Large detectors will be used in the interaction regions to detect and record these interactions. The SSC, through its investigation of fundamental physical processes, will provide new insights into questions of great significance to other sciences as well as to high energy physics. The Super

Collider holds the potential for major, new breakthroughs in science and technology, and is expected to produce discoveries, innovations and spin-offs that will profoundly touch every American.

The FY 1990 budget request for the SSC includes funds to initiate construction. This request builds on five years of research, development, and design efforts and careful planning. While the SSC does extend current technology, its design is backed by a thorough conceptual design and is based on principles and engineering concepts used previously. The Super Collider's design, cost estimates, and schedule have been carefully documented and reviewed. The project is backed by an R&D program specifically related to the SSC that began in FY 1984. Prior efforts in the High Energy Physics program to develop accelerator-quality superconducting magnets, and experience gained in the successful operation of the Tevatron, support the SSC design.

A great deal has been accomplished over the last two years as a prelude to the Department's request for construction funds. For example, the superconducting magnets are the largest SSC technical element, with a requirement for nearly 10,000 superconducting magnets costing over a billion dollars. The ability to manufacture these magnets in a cost-effective manner, with stringent quality control and reproducibility, is critical to the success of the project. In December 1988, the Department announced selection of 16 industrial firms and teams for Phase I of the Magnet Industrialization Program which will aid in establishing an industrial capability to produce the nearly 10,000 superconducting magnets needed for the SSC. During Phase I, which began in January 1989, representatives from the selected companies are observing and discussing superconducting magnet fabrication processes, specification requirements, quality control and testing techniques at the laboratories. A second phase of the industrialization program, scheduled to begin in FY 1989, will provide the opportunity for industrial firms to design, tool and fabricate magnets in a demonstration of their capability to manufacture magnets.

In the area of R&D, significant progress has been made with regard to the magnets and the critical superconducting cable. Long magnets have been fabricated and successfully tested, thus demonstrating proof-of-principle of the current design. Production lots of cable which meet SSC specifications, as well as a prototype fast cabling machine, have already been delivered by industry. The magnet cryostat has successfully met the stringent low heat leak goal set for the SSC. An improved design of the cryosystem required to keep the magnets at their super-cooled temperature has been developed in collaboration with industry.

The site selection was completed with the designation of Texas as the final site on January 18, 1989. Since Congress has not yet given approval to the Department's construction request for the SSC, no irreversible actions will be taken at the site during FY 1989. However, the Department will be moving to locate SSC project management personnel at or near the site quickly to ensure that activities required to be conducted at the site can be carried out in a cost-effective manner. A number of actions have been completed or are underway to ensure an efficient transition to near-term site activities. Chief among these is the Department's selection of the Universities Research Association (URA) to build, manage, and operate the SSC laboratory. A contract was signed on January 18, 1989. Site geotechnical studies and gathering of facts for the Supplemental Environmental Impact Statement will begin shortly. A revised conceptual design reflecting the characteristics of the Texas site and the results of R&D since 1986 will be completed in the fall of 1989. This revised conceptual design, which will include a new bottoms-up cost estimate and schedule, will be reviewed and validated by the Department early in 1990. It is also planned that URA will select on a competitive basis an architect engineer/construction management firm (AE/CM) by the summer of 1989. The AE/CM will prepare the detailed facility design and manage the construction of the conventional facility.

The Department continues to seek substantial financial and scientific participation in the SSC from foreign governments in keeping with the goal of seeking approximately one-third of the cost of the SSC from non-Federal sources. Scientist to scientist contacts will be used to define the technical scope appropriate for collaboration. Once areas of collaboration have been defined, official negotiations between governments will follow and may use the framework of existing agreements where possible. In 1988, the Secretary of Energy sent formal invitations for participation to his six counterparts within the Economic Summit countries. The invitations were followed by several rounds of visits from high-level Departmental delegations to provide SSC status briefings and carry out initial consultations with representatives from the Summit countries. The newly extended Science and Technology agreement with India contains a section on possible collaboration on the SSC. This is a provisional annex for in-kind contributions with a value of $50 million. It is important to note that the Indian agreement is contingent upon a commitment of funds to the project by the U.S. government and the settlement of the remaining issue of intellectual property rights in the umbrella agreement.

Significant funding for the SSC project was provided in FY 1989 for R&D, preliminary design, and site selection activities. We expect continuing R&D achievements throughout FY 1989, including definitive progress on refining and optimizing the design of the superconducting magnets, completion of Phase I and initiation of Phase II of the Magnet Industrialization Program, and increased effort on the injectors and other collider technical systems.

FY 1990 BUDGET REQUEST FOR SUPERCONDUCTING SUPER COLLIDER

The FY 1990 budget request for the SSC is $250.0 million. That request includes $69.0 million for Operating Expenses, $21.0 million for Capital Equipment, and $160.0 million for Construction (Table 4).

The Operating Expenses request will provide for a continued significant level of R&D required to complete the design of the superconducting dipole magnets and to expand the magnet industrialization activities, which I discussed earlier, with particular emphasis on enhancing the reproducibility of the magnets and bringing down the unit cost per magnet. The requested funds will provide for extensive design, prototyping and testing of other collider systems, including the focusing and correction magnets. A significant program of detector R&D will also be carried out as this program moves from the generic phase to focus on specific SSC detectors.

Table 4
General Science and Research
Superconducting Super Collider (SSC)
Budget Authority
($ in millions)

	FY 1988	FY 1989	FY 1990 Request
Operating Expenses	$ 33.0	$ 82.6	$ 69.0
Capital Equipment	-	16.0	21.0
Construction (90-R-106)	-	-	160.0
Total SSC	$ 33.0	$ 98.6	$250.0

The Capital Equipment request of $21.0 million will support the R&D activities on the many accelerator components and systems of the collider and its four injector accelerators, power supplies, test and control instrumentation, and components for the magnet test facility. The budget request also provides for prototypes of detector components as SSC experiments and detectors become more completely defined.

The FY 1990 Construction request of $160.0 million would allow initial construction activities to proceed on a broad front. The request includes $29.0 million for detailed design of technical systems, $29.0 million for detailed design of conventional facilities, $8.0 million for power and utilities distribution systems, $8.0 million for site preparations required to support the overall construction project, $20.0 million for injector technical components which are early critical path items, $30.0 million for long-lead, critical path procurements for collider components (including magnet tooling, superconducting materials, and magnet steel), $28.0 million for project management, support equipment and rental space, and $8.0 million to start on the first on-site shop and support buildings.

NUCLEAR PHYSICS

Nuclear processes determine the characteristics of our universe. Thus, an understanding of nuclei and nuclear phenomena is essential to any basic understanding of the world around us. What we are continuing to learn about nuclei and nuclear processes provides the information base for many areas of technology such as nuclear power, nuclear weapons, and nuclear medicine. The applications are varied and range from the diagnosis and treatment of cancer to the use of nuclear techniques for materials testing and geophysical exploration.

Over the years, many theoretical models have been developed to describe the structure of the nucleus and its behavior. Models in which nuclear matter is treated as a fluid (liquid drop model) or an organized collection of nucleons (shell model) have been well developed. Scientists now know that neutrons and protons, the constituents of the nucleus that are called nucleons, are composed of smaller constituents called quarks. These quarks are bound together in tightly knit groups of threes. However, scientists have not been able to isolate quarks outside the nucleus. It is as if quarks were tied together by elastic strings that cannot be broken. This confining mechanism forms the basis of the newest theory of the strong nuclear force, called quantum chromodynamics (QCD). Many aspects of this theory are addressed both by the Nuclear Physics and High Energy Physics research programs. However, only the Nuclear Physics program emphasizes tests of QCD predictions of quark behavior in nuclear matter. In these tests, individual quarks can be isolated and studied within nuclear matter.

The Nuclear Physics research program is carried out at accelerator facilities around the country. Many of these accelerators are located at the national laboratories, including Lawrence Berkeley Laboratory, Argonne National Laboratory, Oak Ridge National Laboratory, Los Alamos National Laboratory and Brookhaven National Laboratory. Others are at universities such as the Massachusetts Institute of Technology (MIT), Yale University, the University of Washington, Triangle Universities National Laboratory, and Texas A&M. In addition, Nuclear Physics research is performed using nuclear physics facilities in combination with the high energy accelerators at Brookhaven and SLAC.

In FY 1988, three new facilities began operation, and a fourth was being significantly upgraded. At Texas A&M, a superconducting cyclotron, constructed entirely with funding from the State of Texas and private foundations, was started up and will accelerate nuclei to moderate energies of around 100 MeV. Using the cyclotron, scientists will study how light nuclei oscillate when they react. In particular, a phenomenon called monopole oscillation will be studied, in which the nuclei expand and contract like a balloon being blown up. Because these nuclei and their reactions are the basis for nuclear fusion in stars (and in controlled fusion reactions), the data gained will be of great interest to astrophysicists.

The new Yale Tandem Accelerator will use medium-mass nuclei at lower energies of around 10-15 MeV to study extreme deformations of nuclei when they are forced together. Study of these dumbbell-shaped nuclei is expected to provide clues to the nature of the nuclear force and the fundamental way in which two pieces of nuclear matter can join together. For even heavier nuclei, the University of Washington's Superconducting Linac Booster will make available energies of up to 25 MeV which will allow study of the modes of oscillation of massive isotopes. The addition of the South Hall Ring experiment to the Bates electron accelerator at MIT will improve scientists' ability to study the spin components of nuclear particles and structures, characteristics vital to understanding nuclear matter.

The Nuclear Physics program in 1988 generated important research results in a number of fields. One of the most exciting areas was in the newly discovered phenomenon of superdeformed nuclei. Ordinarily, nuclei assume a spherical shape. However, in nuclear collisions, medium-sized nuclei such as barium and cerium can send each other spinning. At high spins, the shape of the nuclei changes into an ellipsoidal (football) shape. Surprisingly, in the deformed state, the nucleus seems to act rigidly, not as a spinning drop of fluid. How the collective action of the protons and neutrons within the nucleus gives it this rigid shape is a major question. Further studies of superdeformed nuclei may give clues as to how the shell model, liquid drop model, and other useful pictures of nuclear structure actually fit together.

An even deeper problem is the question of how the quarks are confined within the protons and neutrons that make up a nucleus. The theory of the quark model, quantum chromodynamics, predicts that at high density or high temperature, a phase change will occur and there will be collective or many particle effects--a quark-gluon plasma (QGP). Finding such a plasma would assist us in understanding and confirming QCD, while studying the plasma will greatly deepen scientific understanding of the nuclear force and phenomena in a new scientific regime.

In an attempt to obtain such a plasma, researchers at Brookhaven National Laboratory are accelerating oxygen and silicon nuclei to energies of 14.5 GeV per nucleon. These collisions produce several hundred particles apiece, and detectors of various types are needed to measure them. As expected, experiments to-date have detected no evidence of a QGP, but the data provides confirmation of the underlying theory. This theory predicts that a transition to a QGP may occur at the 14.5 billion electron volts (GeV) per nucleon energy level or perhaps at the higher energies beyond the capability of our present accelerator facilities.

FY 1990 NUCLEAR PHYSICS BUDGET REQUEST

The FY 1990 budget request for the Nuclear Physics program is $299.3 million. Of that total, $205.3 million is for Operating Expenses, $20.0 million is for Capital Equipment, and $74.0 million is for Construction (Tables 5 & 6).

The Operating Expenses request will provide for continued research in low and medium energy physics, in heavy ion nuclear physics and in nuclear theory at approximately the FY 1989 level. This request focuses on current problems of scientific and technological interest and on exploitation of major new facilities. Special emphasis will be placed on effective use of the upgraded accelerators at the University of Washington and Yale University, the Argonne Tandem/Linac Accelerator System (ATLAS) superconducting heavy ion facility, and the Tandem/AGS high energy heavy ion beams at Brookhaven. Adjustments have been made within the program to accommodate the increased interest of students and postdoctoral fellows in nuclear physics, to enhance the theory component, and to concentrate the program on the highest priorities and new scientific areas.

The FY 1990 Operating Expenses request also includes $11.2 million for R&D in support of research on advanced detector concepts as well as the next generation of accelerators, including the Continuous Electron Beam Accelerator Facility and a relativistic heavy ion collider facility.

Table 5
General Science and Research
Nuclear Physics
Budget Authority
($ in millions)

	FY 1988	FY 1989	FY 1990 Request
Operating Expenses			
Medium Energy Nuclear Physics	$ 83.5	$ 85.7	$ 93.7
Heavy Ion Nuclear Physics	64.9	67.5	71.5
Low Energy Nuclear Physics	24.7	25.9	27.4
Nuclear Theory	10.5	10.8	12.7
Subtotal	$183.6	$189.9	$205.3
Capital Equipment	$ 17.7	$ 18.5	$ 20.0
Construction	41.5	52.5	74.0
Total Nuclear Physics	$242.8	$260.9	$299.3

Table 6
General Science and Research
Nuclear Physics
Budget Authority
($ in millions)

Construction

Project	TEC	FY 1989	FY 1990 Request
Accelerator Improvements and Modifications (90-R-201)	$ 4.6	$ -	$ 4.6
Accelerator Improvements and Modifications (89-R-201)	4.3	4.3	-
General Plant Projects (GP-E-300)	4.4	-	4.4
General Plant Projects (GP-E-300)	3.7	3.7	-
Continuous Electron Beam Accelerator Facility (87-R-203)	265.0	44.5	65.0
Total Construction		$ 52.5	$ 74.0

The Capital Equipment request of $20.0 million will improve overall instrumentation for effective use of the research facilities. These funds will provide for such equipment as particle detection systems and data acquisition and analysis systems. The FY 1990 request will also provide for general purpose equipment to meet the laboratory-wide needs of Lawrence Berkeley Laboratory.

The FY 1990 Construction request for Nuclear Physics is $74.0 million. Of that amount, $65.0 million is for the Continuous Electron Beam Accelerator Facility (CEBAF). CEBAF will provide access to the critical transition region between the traditional and emerging descriptions of nuclear matter. The main research tool at CEBAF will be an accelerator with electron beams in the energy range of 0.5 to 4.0 GeV. Its combination of high-energy, high-current, and continuous beam with excellent beam quality is expected to revolutionize the production and collection of data for nuclear physics research.

The FY 1990 Construction funds for CEBAF will be used to begin construction of the experimental area, continue construction of the accelerator enclosure, continue procurement of cavities and cryostats, and begin procurement of arc magnets, power supplies and of experimental equipment.

The Construction request also includes $4.6 million for Accelerator Improvements and Modifications (AIM) and $4.4 million for General Plant Projects (GPP). AIM projects address the need for improvements to research accelerators and related experimental facilities while GPP projects focus on general laboratory facilities. These projects are needed to maintain the scientific effectiveness, reliability, and efficiency of Nuclear Physics facilities.

GENERAL SCIENCE PROGRAM DIRECTION

The FY 1990 budget request for General Science Program Direction is $3.9 million. This request is under the General Science and Research appropriation. The requested funds are required to provide for the salaries, benefits, travel, and other expenses associated with 49 full-time equivalent employees in the Office of High Energy and Nuclear Physics and associated support staff required to administer these programs.

Now let me turn to the programs under the Energy Supply R&D appropriation. The first of these is Basic Energy Sciences.

BASIC ENERGY SCIENCES

The Basic Energy Sciences (BES) program supports the research that provides the fundamental scientific and engineering knowledge needed to underpin advances in DOE's applied technology programs and in industry. In addition to supporting research over a wide range of technical areas, the BES program provides support for the operation of major user facilities such as synchrotron radiation sources, neutron sources, and a combustion research facility. These facilities are national facilities maintained and operated for the benefit of all qualified researchers from Government laboratories, academia, and the industrial laboratories. These facilities are critical to continued progress in the scientific research supported by the BES program; and they are playing an increasingly important role in research carried out by the private sector.

Currently, BES has seven major facilities built and operating. In the past, the demand for time at these facilities has been greater than the time that could be made available. The FY 1990 budget request, which I will discuss later in this statement, provides a more optimal operating level for these facilities and reflects the program's responsibility to plan for the future by constructing advanced scientific facilities which will provide needed new capabilities for conducting forefront research. The 6-7 GeV Synchrotron Radiation Source, which has been rated as a top priority by several science panels, is just such a facility; and I will discuss it in more detail later in my statement.

BES supports about 1,400 individual research projects conducted by about 4,000 scientists at more than 200 separate institutions. The BES program undertakes research primarily at national laboratories and universities and, to a lesser extent, at private research laboratories and other Government laboratories. Approximately 60 percent of BES-supported research is carried out at national laboratories. Since these laboratories are also involved in many aspects of the applied energy programs, they are skilled at facilitating transfer of scientific information and technology from experiment to application. In addition, the stability of the organization and the specialized capabilities which exist at the laboratories in many instances are unmatched. The BES program also provides significant direct funding support to universities in addition to providing them with research time at the various user facilities located at the national laboratories. BES also provides funding for the supercomputer centers used by other ER investigators as well as BES researchers. All of these BES-supported activities play a major role in training future scientists.

In addition to universities and national laboratories, BES supports research in and maintains strong ties with industry. Representatives from different industries serve on the BES Advisory Committee; experts from industry participate in the review of research proposals and use the specialized facilities sponsored by BES; industrial scientists participate in advisory committees to the national laboratories; and industry representatives are invited to attend BES conferences and workshops on special topics.

The FY 1990 budget request for Basic Energy Sciences is $590.0 million (Table 7). The request includes $470.7 million for Operating Expenses, $37.6 million for Capital Equipment, and $81.7 million for Construction.

The request for Operating Expenses will allow us to conduct research aimed at uncovering new energy-related information in materials sciences, chemical sciences, applied mathematical sciences, engineering and geosciences, energy biosciences, and advanced energy projects. The funds are necessary to provide for continuation of the core research programs at the present level, expanded research activity in high temperature superconductivity and molecular sciences, and continued operation of the important national scientific user facilities. These funds will also provide for research and development required for further progress on planned new advanced scientific facilities such as the 6-7 GeV Synchrotron Radiation Source, which will provide hard

Table 7
Energy Supply R&D
Basic Energy Sciences
Budget Authority
($ in millions)

	FY 1988	FY 1989	FY 1990 Request
Operating Expenses			
Materials Sciences	$ 170.4	$ 180.5	$ 202.3
Chemical Sciences	127.2	132.9	147.9
Applied Mathematical Sciences	41.9	42.7	44.9
Engineering and Geosciences	31.0	33.2	34.1
Advanced Energy Projects	14.1	14.5	14.9
Energy Biosciences	20.0	20.6	21.2
Program Direction	4.5	4.9	5.4
Subtotal	$ 409.1	$ 429.3	$ 470.7
Capital Equipment	31.7	31.7	37.6
Construction	116.9	88.3	81.7
Total Basic Energy Sciences	$ 557.7	$ 549.3	$ 590.0

X-rays for frontier research in a variety of areas including the surface characteristics of materials, the structure of biological materials, and angiography.

I would now like to describe briefly some of the research activities and results of each of the major BES subprograms. In FY 1990, research in Materials Sciences will continue to increase our understanding of the properties and behavior of materials. Materials often are the limiting feature in energy systems because they often prevent higher temperatures and efficiency and shorten the lifetime of systems. Conversely, advances in materials frequently provide new opportunities, which can ultimately result in entirely new technologies, such as high temperature superconducting instruments and electrical machinery. These technologies in turn depend on the discovery and development of suitable superconducting materials.

BES-supported Materials Sciences research concentrates on selected areas of importance and on areas where problems are known to exist or are anticipated. Some research may relate to a single energy technology (such as research on photovoltaic materials for converting solar energy to electricity); some applies to many technologies (such as research on corrosion mechanisms in materials); and still other work has more fundamental implications for all materials research (such as research on interfaces in solids and computer modeling of materials phenomena).

During the past year, Materials Sciences continued to make substantial progress on the new high temperature superconductors. Researchers at Argonne National Laboratory were able to isolate a pure compound in the bismuth-strontium-calcium-copper-oxygen mixture which was superconducting at a temperature of -163 degrees Centigrade. Previously, similar results occurred at temperatures of up to only -188 degrees Centigrade. This was also an especially noteworthy achievement because this compound does not include any of the expensive rare earth elements.

In another accomplishment related to superconductivity, scientists at Sandia National Laboratories successfully prepared the first thin films of the new thallium-based superconducting materials and showed that they can carry high critical currents which are not easily destroyed by a magnetic field. This is important because the goal is to achieve sufficiently high currents without exceeding the critical level, because to do so would eliminate the superconducting effect.

Synchrotron radiation sources and neutron sources supported by BES continued to be used by the national scientific community to determine the structure of the many high temperature superconductors which have been discovered. Other advances in Materials Sciences included work on enhanced methods for forming structural ceramics, a class of materials which have potential for use in such applications as automobile engines. Researchers also developed computer models of structural features called grain boundaries, which often are the weakest area in a material.

In Chemical Sciences, research supported in FY 1990 will continue to expand our knowledge, with a long-term goal of creating new or improved processes for using and converting energy resources. The focus of the research is on the chemical properties of matter and the interactions of the component molecules, atoms, ions, and electrons. Research in some areas, such as chemical catalysis, where scientists seek a better understanding of why some molecules uniquely promote specific chemical reactions, may be quickly exploited by industrial process designers. Research in other areas, such as the use of light-induced chemical reactions to produce hydrogen from water decomposition, may not find practical application for a number of years. Still other research areas, such as the study of the interactions of atoms and electrons with plasmas, may produce knowledge important in the development of entirely new energy technologies.

Chemical Sciences research ultimately affects such areas as production of fuels and chemicals from coal, nuclear waste separation, conversion of biomass (such as wood and leafy materials) into liquid fuels using enzymes or micro-organisms, and detection and measurement of harmful byproducts of energy processes. One research area that will continue to receive emphasis in FY 1990 is coal science. Scientists are working to understand the structure and chemical reactivity of coal. Such understanding will aid in developing new ways of transforming coal into synthetic gaseous or liquid fuels. A second principal focus in FY 1990 will be on combustion. With world energy production still overwhelmingly based on fossil fuels, control and reduction of pollution depends largely on improving the efficiency with which such fuels are burned. To do this requires a broad and basic understanding of the complex process of combustion. The FY 1990 Operating Expenses request will also provide for the study of heavy elements and the production and separation of heavy radioactive elements. These latter areas are of great importance to radioactive waste disposal programs.

During the past year, research in Chemical Sciences demonstrated for the first time a new measuring technique which is capable of providing direct information about the structure of molecules on surfaces. Also, scientists have confirmed a theory concerning the existence of the Antarctic ozone hole which improves our understanding of the effects of chemical pollutants on changing atmospheric conditions. Other accomplishments include development of new, high-power lasers for use in scientific research, improved models for predicting the behavior of coal solutions, and the discovery of a new method of using electric fields to stir liquids in chemical separations processes, thus increasing the efficiency of extracting the desired chemical and significantly reducing energy consumption.

Research in Engineering and Geosciences encompasses two broad disciplines. The first, engineering research, aims to enlarge our knowledge of the processes and concepts that underlie current engineering practice in energy technologies. Although fundamental engineering research is aimed at long term goals, important near-term applications have emerged from the work supported. For example, the Department has supported research into new optical theories that aim to maximize the intensity of light over an area, rather than to produce a sharply focused image. Such theories recently have allowed the Department's solar energy program to construct solar furnaces that concentrate the sun's light by 50,000-fold, a tripling of previous performance. This gives light intensity nearly equal to that at the sun's surface. One possible application is to power lasers for communication from satellites. The increased intensity now possible would reduce the problem of energy losses encountered when sunlight is converted to electricity.

Research in geosciences contributes to a better understanding of the energy-related aspects of geological, geophysical, and geochemical processes related to the earth. Several geoscience research projects that will continue in FY 1990 involve advanced methods for remote sensing and measurement of the properties of earth materials as well as direct scientific drilling to study the thermal zones of the earth and those bearing fossil fuels. For example, high resolution underground acoustic imaging is being developed to illuminate, in detail, the characteristics of groundwater and petroleum reservoirs.

In another geosciences project, drilling was started in July 1988 in the Valles Caldera, New Mexico, on the tenth Continental Scientific Drilling Project funded by DOE. This 5,780-foot hole is now complete and is the deepest fully cored hole in a geothermal area thus far drilled in the United States. The project, which will continue in FY 1990, will study boiling, heat transfer, and ore deposition in thermal zones of the earth.

Applied Mathematical Sciences supports long-range mathematical and computational research. Computational methods provide insights into experimental data and simulate ideal experiments for testing analytical models. This subprogram advances all aspects of scientific computation--from proofs of theorems about mathematical equations to understanding the structure of parallel computer systems.

In 1988, scientists at Sandia National Laboratories in Albuquerque discovered new methods for solving complex scientific problems at unprecedented speeds. They developed innovative methods for parallel computing for the

1,024-processor hypercube computer at Sandia and demonstrated speedups over single-processor computing near the theoretical limit on full-scale applications in wave propagation, solid mechanics, and fluid flow research.

In addition to supporting mathematical and computational science research in FY 1990, Applied Mathematics Sciences will provide access to scientific supercomputers which are essential for researchers supported by the Office of Energy Research in high energy and nuclear physics, basic energy sciences, and biological and environmental research.

Energy Biosciences research in FY 1990 will continue to increase our understanding of basic biological principles, biological mechanisms, and organisms useful for energy applications, such as the production of fuels from biomass materials. Other possible applications include biological processes that might substitute for energy-intensive activities, such as fertilizer production, and biological means of resource recovery, such as the extraction of copper from ore through leaching. This research activity consists principally of research in the plant and microbial sciences.

One recent research accomplishment concerns a small plant, *Arabidopsis*, that is having a profound effect on plant science research since recent DOE-supported research showed how easily mutations in the plant could be induced, recognized and employed for studying many different plant processes. As a result, *Arabidopsis* is now the most commonly used plant system for basic biological research. The studies are done using molecular biological techniques. The most important aspect of this discovery is the ability it gives us to identify and subsequently transfer genes having agronomic importance from Arabidopsis to certain crop plants. An example of this would be the transfer of herbicide resistance genes. Ultimately, the biotechnology industry may be able to take advantage of this ease of mutation to produce much improved plants.

The results of microbiological research about fermentations, plant-microbe interactions, and the microbial degradation of plant material will be the foundation of new biosystems capable of producing fuels and chemicals. Bacteria may be able to do work now done by energy-consuming industrial processes. Recent studies in this direction have involved sulfate-reducing bacteria that convert the sulfur in sulfuric acid to hydrogen sulfide. Recently, scientists have discovered a virus that can transfer genetic material between different strains of these sulfate-reducing bacteria. This opens up the possibility of tailoring these bacteria to perform such tasks as removing sulfates from waste streams. Genetically altered, sulfate-reducing bacteria provide insights into bacterial conversion processes applicable to many industrial applications, to bacterial-induced corrosion processes and to preservation of oil wells, where hydrogen sulfide production interferes with drilling operations and ultimately in the oil refining process itself.

The main objective of Advanced Energy Projects is to explore the feasibility of novel energy-related concepts evolving from basic research that are at an early stage of scientific definition and therefore not yet of practical use to technology programs. Although these concepts involve a high degree of risk, they have the potential for high payoffs. Thus, Advanced Energy Projects fills a void, encouraging the transition of basic research results into technological applications. It also supports exploratory projects that do not precisely fit into any single program area but that have broad potential applications.

Recently a small company, ParaMagnetic Logging, Inc., which is supported by Advanced Energy Projects, established the feasibility of a new technique to be used for locating oil and gas deposits overlooked in existing reservoirs. Previously, it had been impossible to systematically locate such deposits without drilling expensive new holes. The oil and gas industry now uses measurements of the electrical resistivity of geological formations to determine the presence of oil and gas in newly drilled holes. However, such measurements have been impossible to make through steel pipes that surround wells in existing older oil fields. The company has developed a new technique that will allow such measurements to be made, thus resulting in the detection of "missed oil" and "by-passed gas." This technology has the potential to significantly increase the proved oil reserves within the United States.

Advanced Energy Projects also supports research on heavy-ion accelerators aimed at validating this technology as an inertial fusion driver. Heavy-ion beams could be the ultimate choice for generating energy from inertial fusion if issues associated with accelerating beams to the required intensities are successfully resolved with cost-effective accelerator designs. Scaled experiments aimed at addressing these issues are underway and will be continued.

As I mentioned earlier, the operation of major user facilities is a very important part of the BES program. At the present time, BES has seven major facilities: the National Synchrotron Light Source (NSLS) at Brookhaven National Laboratory (BNL); the Stanford Synchrotron Radiation Laboratory at Stanford University (SSRL); the High Flux Beam Reactor (HFBR) at BNL; the High Flux Isotope Reactor (HFIR) at Oak Ridge National Laboratory; the Intense Pulsed Neutron Source (IPNS) at Argonne National Laboratory; the Los Alamos Neutron Scattering Center at Los Alamos National Laboratory; and the Combustion Research Facility at Sandia National Laboratories (Livermore). It is important to keep the facilities operating at levels which are responsive to the demand for their use; and the FY 1990 budget request is consistent with this goal.

During the past year, many new and interesting results have come out of research at these facilities. For example, at SSRL, synchrotron radiation was demonstrated to be effective in imaging the coronary artery circulation of human subjects. Clinical research in this area will now move to the NSLS because of the higher flux of radiation available there. Also at the NSLS, lithography experiments by IBM have led to full-scale working computer chips with feature sizes as small as one-half micron, which is one thousandth of a millimeter. The success of these has made synchrotron radiation a leading candidate for fabricating computer chips with feature size at or below one-half micron. At the IPNS and the HFBR, neutrons were used to uncover the structure of the new high temperature superconductors.

The HFIR has been undergoing reviews and modifications since the pressure vessel was observed to have higher than expected radiation embrittlement in November 1986. These reviews are now nearing completion and a plan has been established for restarting the reactor using lower pressure, higher temperature, and lower power.

The BES Capital Equipment request for FY 1990 is $37.6 million. These funds will ensure that BES-supported researchers will continue to have the equipment needed to initiate and continue advanced research, much of which involves experiments at extremes of temperature and pressure. Reliable, precise measurements under these conditions challenge the current state-of-the-art. Replacements and new equipment funded under this request, which are important to the continued success of the program, include such items as electron microscopes, neutron spectrometers, molecular beam equipment and computers for equipment control and data analysis.

In addition to its seven facilities, BES is constructing advanced scientific facilities which are critically needed by the scientific community. A substantial part of the FY 1990 construction request of $81.7 million for BES (Table 8) is for the 1-2 GeV Synchrotron Radiation Source at Lawrence Berkeley Laboratory and the 6-7 GeV Synchrotron Radiation Source at Argonne National Laboratory. Both facilities are needed to cover the entire radiation range needed for experiments supported by the program. The 1-2 GeV project is a third generation light source in the vacuum ultra violet/soft x-ray range. The FY 1990 request includes $26.0 million for this project, for which construction was begun in FY 1987. The 6-7 GeV project is a new hard x-ray synchrotron light source which has been rated as top priority by several

Table 8
Basic Energy Sciences
Construction Summary
($ in millions)

Project Title	FY 1989	FY 1990 Request	TEC
Neutron Scattering Experimental Hall (LANL)	$ 4.0	$ 0	$ 17.5
1-2 GeV Synchrotron Radiation Source (LBL)	25.0	26.0	99.5
3 GeV SPEAR Injector (SSRL)	6.5	4.5	14.0
6-7 GeV Synchrotron Radiation Source (ANL)	6.0	40.0	456.0
General Plant Projects	4.9	5.6	5.6
Accelerator Improvement Projects	2.3	4.6	4.6
Partial Decontamination and Medium Term Protective Storage of the ANL CP-5 Research Reactor	0	1.0	5.4
Congressionally Initiated University Projects	39.6	0	0
Total Basic Energy Sciences Construction	$ 88.3	$ 81.7	

science panels, including one from the National Academy of Sciences and one from the DOE Energy Research Advisory Board. Funding for design only was included in the FY 1989 appropriation. The FY 1990 Construction request includes $40.0 million for initial construction.

BIOLOGICAL AND ENVIRONMENTAL RESEARCH

The Biological and Environmental Research (BER) program develops the knowledge needed to identify, understand, and anticipate the long-term health and environmental consequences of energy use and development. Another objective of the program is to use the Department's unique scientific and technological capabilities to solve major scientific problems in medicine and biology.

To achieve these objectives, the BER program supports research in a number of areas, including biology and life sciences; nuclear medicine; environmental sciences, including carbon dioxide and global change-related research; as well as research to develop advanced measurement techniques used in research throughout the program. The FY 1990 budget request for BER is $271.4 million (Table 9). Funding by research area is shown in Table 10. Of that amount, $255.4 million is for Operating Expenses, $12.5 is for Capital Equipment, and $3.5 million is for Construction. I would like to comment on each of the research areas provided for in this request.

The biological research carried out under the Health Effects activity is aimed at providing a sound data base for evaluating human health risks, such as cancer and genetic effects, that could result from chronic, low-dose exposures to energy-related environmental agents, such as ionizing radiation

Table 9
Energy Supply R&D
Biological and Environmental Research
Budget Authority
($ in millions)

	FY 1988	FY 1989	FY 1990 Request
Operating Expenses	$202.4	$225.0	$255.4
Capital Equipment	8.5	9.0	12.5
Construction	56.2	23.5	3.5
Total	$267.1	$257.5	$271.4

Table 10
Energy Supply R&D
Biological and Environmental Research
Budget Authority
($ in Millions)

	FY 1988	FY 1989	FY 1990 Request
Operating Expenses			
Late Effects on Health & Environment of Energy Development and Use			
Analytical Technology	$ 15.2	$ 14.8	$ 16.2
Environmental Research	31.1	36.6	43.6
Health Effects	66.2	69.3	72.2
General Life Sciences	38.4	46.1	60.4
Medical Applications			
Medical Applications	24.1	30.2	32.3
Boron Neutron Capture Therapy at INEL	9.8	6.4	5.3
Carbon Dioxide Research	13.6	17.6	20.8
Program Direction	4.0	4.0	4.6
Subtotal	$202.4	$225.0	$255.4
Capital Equipment	8.5	9.0	12.5
Construction	56.2	23.5	3.5
Total	$267.1	$257.5	$271.4

and toxic chemicals. This research includes epidemiological, laboratory animal, cellular, and molecular level studies. The FY 1990 request of $72.2 million will provide for increased emphasis on molecular-level studies and research to correlate the relationships between exposure to chemicals or radiation and the consequent biological response. The FY 1990 request will also allow us to extend ongoing health surveillance studies of DOE workers to additional facilities. Research in FY 1990 will emphasize chemical and physical characterization of potentially significant DNA damage from high linear energy transfer radiation, such as alpha and neutron radiation.

Our growing knowledge in the Health Effects area will help us to better understand such important topics as the possible hazards of indoor radon exposure. The radon research program was begun in 1987 to study this major national public health concern. Now composed of 60 projects carried out at various academic institutions and national laboratories, the program will continue to emphasize the basic scientific research needed to understand and predict the health risk related to exposure to indoor radon. This research includes measuring the exposure to radon more accurately, better defining health risk estimates, and determining the role of radon exposure in combination with smoking in causing lung cancer.

The FY 1990 request for General Life Sciences research of $60.4 million will emphasize molecular and cellular studies to examine how key biological processes operate. With this knowledge, researchers can learn more about how physical and chemical agents affect living processes. The research also identifies early indicators of biological damage, teaches us how cells repair themselves, and leads to new techniques for analyzing biological damage.

Ultimately, we hope to understand the basis for individual sensitivity to radiation and environmental chemicals, and to have the technology and data base to define risks and allow the establishment of improved health protection standards.

In FY 1990, a major focus in General Life Sciences research will be in molecular genetics to reveal DNA damage and repair mechanisms and to understand the human genome--the complete set of genetic instructions that make a human being. The Department's human genome effort is focused on developing new instrumentation and methodologies that will make such research far less costly to perform per fragment of DNA identified, as well as far less labor intensive. New insight is likely to come from a recent achievement by BER-supported investigators at the Massachusetts Institute of Technology. The new technology developed there permits production of many copies of damaged DNA, resulting in the capability to rapidly detect mutational changes with an error rate of one in a billion, and thus provides unprecedented sensitivity in quantifying DNA damage in people exposed to hazardous chemicals and radiation.

One important byproduct that results from all the areas of BER research is the development of instrumentation. During the early stages of science, observation with available instruments is usually sufficient. However, at some point, it becomes necessary to measure events that have not been measured before. New technologies often result in new, more efficient, or more cost effective instruments that find applications outside the original thrust of the research. For example, the flow cytometer, developed initially to sort and count blood cells, has been improved progressively to the point where it now quickly isolates relatively pure samples of each of the individual chromosomes, thus, providing important technologies for the human genome project. In another significant advance, a team of scientists from the Lawrence Livermore National Laboratory and the Lawrence Berkeley Laboratory has used the scanning tunneling microscope to obtain the first direct images of unaltered double-stranded DNA. Extensions of this work could have major ramifications for molecular biology and research on biological structures.

Coordination of the human genome research and technical activities between DOE and the National Institutes of Health was formalized in a 1988 Memorandum of Understanding, which provides for a joint advisory committee and an interagency working group. Research on the human genome at Los Alamos and Lawrence Livermore National Laboratories and Lawrence Berkeley Laboratory will build upon the considerable progress already made in developing the intellectual and physical resources (including engineering, computational and biomedical expertise) for genome analysis. For example, the molecular nature of chromosome ends and centers has been identified, contributing substantially to our ability to map chromosomes. In addition, libraries of cloned DNA have been prepared, and an automated system is now rapidly processing the DNA in one chromosome-mapping effort. The FY 1990 request of $27.6 million for the genome activity, up from $17.5 million in FY 1989, provides for an expanded effort which will include initiating physical mapping efforts for chromosomes where this is not already underway, testing new approaches for mapping, and providing necessary support to DOE Human Genome Centers and other large interdisciplinary efforts. Pilot sequencing projects will also be initiated to evaluate and compare new sequencing technologies. The proposed level of effort will enable the Department to maintain its momentum in the area of developing new tools and methodologies which will greatly increase the pace and lower the cost for mapping and sequencing the human genome.

The FY 1990 request for the Analytical Technology activity is $16.2 million. This request will provide for research directed at developing new and more sensitive instruments to make physical, chemical and biological measurements of energy-related emissions. These instruments will also assist scientists in measuring human exposure and biological response to those emissions.

Over the years, the Department's Medical Applications research has been at the forefront of important medical discoveries and has provided information that has led to the widespread use of nuclear technology in modern clinical and laboratory medicine, opening avenues of diagnosis and treatment that otherwise would not have been available. The FY 1990 budget request of $37.6 million will allow research to continue on the development of new radioisotopes, clinical procedures, and instrumentation.

The effort in boron neutron capture therapy (BNCT) has been greatly expanded in the last two years. An effort is currently under way to develop BNCT for the treatment of brain tumors and melanomas. This method is based on the fact that the reaction of an isotope of boron with neutrons produces a nuclear reaction that provides a form of highly localized radiation. When used with boron compounds that selectively concentrate in tumors, this method may make it possible to deliver therapeutic doses of radiation to a tumor with minimal effects on adjacent healthy tissues. Ongoing laboratory research is aimed at providing the data base necessary to determine whether this type of therapy can reasonably be expected to be successful with patients.

The development and use of radiopharmaceuticals in metabolic studies is another major aspect of the program. Radiopharmaceuticals are radioactively labeled compounds that concentrate in certain body organs and can be imaged with sensitive radiation detectors to make the body's internal organs visible to medical diagnosticians. Changes in the spatial and temporal distributions of the labeled substances allow diagnosis to be made without invasive procedures such as biopsy. Radiopharmaceutical research emphasizes studies of brain and heart metabolism, but also contributes to diagnosis and therapy involving other organs. The FY 1990 request also provides for clinical feasibility research which includes testing of new radiopharmaceuticals in animals and, subsequently, in selected patients. This effort evaluates methods for studying, diagnosing, and treating diseases such as cardiopulmonary disease, mental disorders, cancer, and metabolic disorders.

Research will also continue in FY 1990 on the advanced instrumentation required to diagnose and treat patients using nuclear medicine. This research emphasizes advanced detector studies, improved resolution of Positron Emission Tomography (PET) and other imaging techniques, and innovative applications of energy-related technology for noninvasively studying the functioning and condition of internal organs. Recently, in this area, the development of a new and unique PET system has been completed. PET imaging is a method for studying the metabolism of radioactively labeled substances in selected parts of the body by tracking the radioactivity of the label. The new system significantly improves the quality of the computed images and makes it possible to acquire an essentially three-dimensional picture of an area as large as a human heart. This new PET system will be used to study blood flow in the heart and may be able to detect coronary artery disease before clinical symptoms appear. It can also help to diagnose the focal points in the brain that cause seizures in children and distinguish among the different kinds of mental degenerative diseases. In FY 1990, research will continue to develop a technique called high field nuclear magnetic resonance spectroscopy for studying metabolic functions in organs. If successful, it would allow the diagnosis of metabolic disorders of organs such as epilepsy and dementias. Research will also focus on using synchrotron radiation for a safer coronary angiography test which would be used to study the development of atherosclerosis before it has become severe enough to cause a heart attack.

The FY 1990 budget request for Environmental Research is $43.6 million. Another $20.8 million in Operating Expenses and 1.5 million in Capital Equipment is requested for the Carbon Dioxide research activity. Together, these research activities provide information needed to address a number of important issues, including acid rain, global climate processes, the resiliency of ecosystems and marine coastal areas to energy activities, and management of energy and weapons related radiation wastes.

The request for Environmental Research will provide for continued research aimed at determining how energy-related agents move through and are changed by the atmosphere, oceanic and terrestrial ecosystems, and how they ultimately affect humans. Scientists are also looking at how the environment itself may be affected and how negative effects can be mitigated.

An increased research activity in FY 1990 will be aimed at developing basic scientific theories about the interrelationships between atmospheric and terrestrial systems (e.g., plants, water, soil, and air). In the future, we plan to test these theories in natural environments, primarily at the Department's National Environmental Research Parks. The information gained through a combination of theory and experiments will contribute to a better understanding of global change processes.

An ongoing effort in subsurface sciences will be expanded from $7.3 million to $11.8 million in FY 1990 to develop a better research base for cost-effective management and remediation of contamination from the Department's energy- and weapons-related project wastes. The recent discovery by BER-supported researchers of microbial communities over 1700 feet deep within the earth has changed scientific thinking about microbial abundance and diversity at such depths. Previously, such diversity and abundance was believed to be common only at depths of tens of feet. This discovery may contribute to cost effective _in situ_ cleanup of deep groundwater contamination. Technology transfer of the newly discovered microorganisms to the pharmaceutical industry is already underway. Work supported in FY 1990 will focus on understanding of the factors that influence the mobility of mixed wastes through the soil so that shallow, low-level waste burial sites can be operated more safely and cost effectively.

Another major area of emphasis in FY 1990 relates to carbon dioxide and the greenhouse effect which, linked to global climate change, has become a national and international science and energy policy issue. A major component of the global change activity is the Carbon Dioxide program, which I previously mentioned. It is continuing to develop sound, quantitative information about atmospheric CO_2 and other energy emissions and their effect on climate and earth resources. We must improve our understanding of the cause and effect relationships between CO_2 concentrations and global, regional, and seasonal warming to support energy policy recommendations. This program is focused on the scientific aspects of the problem.

The FY 1990 budget request will support research aimed at improving our knowledge base in the above areas, with particular emphasis on the key issue of determining the quantitative links between the rise in concentrations of greenhouse gases in the atmosphere and global warming. Specifically, we must have a better understanding of how climate is changing and whether those changes are related to atmospheric greenhouse gases. Substantial improvement is needed in the way oceans and clouds are portrayed in climate models in order to accurately predict changes. Finally, we need to collect data to evaluate the primary effects of both CO_2 and climate change on biological systems as a whole.

The program will seek multidisciplinary input from the scientific community to gain a broad perspective. Climate change research activities throughout the Department are being coordinated by Energy Research, with interagency coordination being implemented through the Committee on Earth Sciences of the Federal Coordinating Council for Science, Engineering, and Technology. International activities are being coordinated through such organizations as the Intergovernmental Panel on Climate Change.

The FY 1990 request for Program Direction is $4.6 million. These funds are required to pay for the personnel costs of 55 full-time equivalents to direct, manage, and support BER's multidisciplinary research efforts.

The FY 1990 BER budget request for Capital Equipment and Construction is $16.0 million. This includes $12.5 million for Capital Equipment and $3.5 million for Construction. The Capital Equipment request supports replacement of deteriorated and obsolete equipment at the laboratories as well as acquisition of new, state-of-the-art equipment, particularly for the carbon dioxide and subsurface sciences activities. The General Plant Projects that fall under the Construction request are necessary to keep the general plant of the laboratories (e.g., buildings, roads and utilities) in good operating condition.

Now let me turn to the several ER programs that are related to my statutory responsibilities to advise the Secretary on DOE's research and development programs, on the non-weapons multiprogram laboratories, and on the education and training of scientists and engineers.

ENERGY RESEARCH ANALYSES

The technical assessments and research analyses carried out under the Energy Research Analyses (ERA) program cover all aspects of the Department's civilian R&D programs. These in-depth reviews provide information on the quality of the research and its impact, as well as on opportunities for future research. In addition, the program plays a key role in coordinating the Department's involvement in acid rain research. Specifically, the program provides funds under the National Acid Precipitation Assessment Program (NAPAP) for summarizing the state-of-the-science in acid rain research and producing assessment reports useful for policymakers.

The FY 1990 budget request of $4.0 million will provide funding for peer reviews of projects in the Office of Energy Research and also in Conservation and Renewable Energy, and Fossil Energy. It will also provide for technical assessments which will be performed in areas such as advanced polymer composites, corrosion, and plastics recycling. In addition, funds will be provided to help complete the final NAPAP Assessment, including support for synthesizing the results of over $500 million worth of Federally funded acid rain research through FY 1990.

UNIVERSITY RESEARCH SUPPORT

The Nation's colleges and universities produce much of the new scientific knowledge and technical talent that ultimately contributes to the Nation's economic and social progress. For this reason, the Department supports close interactions with the Nation's universities through direct support for on-campus research, collaborative efforts involving DOE laboratory scientists and facilities, and the specific research and science education-related programs that I will discuss next, the University Research Support program and the University Research Instrumentation program. These programs help strengthen university capabilities to perform long-range R&D, contribute to the quality and numbers of young people pursuing energy R&D careers, and enhance technology transfer to the private sector.

The FY 1990 budget request for the University Research Support (URS) program is $16.1 million. The four URS activities include Laboratory Cooperative Science Centers, R&D Laboratory Technology Transfer, University Reactor Fuel Assistance, and Energy Manpower Development (Table 11).

The first URS subprogram, Laboratory Cooperative Science Centers, includes support for faculty and student research and training appointments at the DOE laboratories. The essential roles played by the Department's laboratories in assisting in university research and manpower development have been strongly endorsed in reports of the Energy Research Advisory Board (ERAB) and White House Science Council on the health of U.S. colleges, universities and industry. The recent ERAB report to the Secretary on the Department's role in science education (August 1988) states that "perhaps the Department's greatest contribution to education is in providing a link between universities and the laboratories." Consistent with these analyses, the URS program continues to provide expanded support for lab-based education activities. A major initiative begun in FY 1988 provides support for semester-length appointments for undergraduate science and engineering students selected nationally based on academic merit and future research interests. These appointments will be provided to approximately 450 students and to about 36 faculty/student research teams at six laboratory science centers. Support will also be provided for some 2,000 summer faculty/student research appointments.

It is broadly accepted that person-to-person interactions are the keys to successful technology transfer. The R&D Laboratory Technology Transfer subprogram aims to make the results of Federally funded R&D more widely available in the marketplace by cost-sharing the assignment of industry

Table 11
Energy Supply R&D
University Research Support
Budget Authority
($ in millions)

	FY 1988	FY 1989	FY 1990 Request
Operating Expenses			
Laboratory Cooperative Science Centers	$ 13.6	$ 15.4	$ 9.4
R&D Laboratory Technology Transfer	1.1	1.4	1.7
University Reactor Fuel Assistance	1.7	2.2	2.3
Energy Manpower Development	1.8	2.7	2.7
Total University Research Support	$ 18.2	$ 21.7	$ 16.1

scientists at multiprogram and major single-purpose laboratories and by supporting pilot technology transfer initiatives. In FY 1990, 30 industry-laboratory technology exchange assignments involving personnel from both large and small companies will be funded.

University Reactor Fuel Assistance provides support associated with the fabrication and shipping of nuclear fuel for university-based nuclear research and training reactors. The FY 1990 budget request will provide for refueling five reactors and will fund 20 reactor sharing research awards involving scientists from more than 50 universities. Continued support also will be provided to the low enriched uranium (LEU) reactor fuel conversion program required by the Nuclear Regulatory Commission. By FY 1990, 11 nuclear reactors will have either converted, or will be in the process of converting, to LEU.

DOE, along with other science agencies, depends on a continual flow of new, highly educated scientists and engineers for its current and future programs. The last URS subprogram, Energy Manpower Development, carries out a number of activities directed at increasing the number of young people pursuing energy-related scientific and technical careers. For example, the FY 1990 budget request will support approximately 23 Pre-Freshman Engineering projects reaching 2,200 students, as well as seven high school science student honors programs. The latter will be conducted at the Lawrence Livermore, Brookhaven, Argonne, Lawrence Berkeley, and Oak Ridge National Laboratories, Fermi National Accelerator Laboratory, and the Pacific Northwest Laboratory. At least eight DOE laboratories, including the seven I just mentioned and Los Alamos National Laboratory, will also sponsor summer research programs for approximately 150 high school science teachers selected on a national basis.

UNIVERSITY RESEARCH INSTRUMENTATION

University scientists conducting research for DOE need access to state-of-the-art research instrumentation. The Department's University Research Instrumentation (URI) program helps universities acquire the state-of-the-art instrumentation that is needed to conduct long-range research on energy problems. In the process, the program provides graduate students with hands-on experience in the use of this sophisticated research instrumentation. This annual, competitive program involves substantial cost-sharing from the institution receiving the equipment. Each year the Department's program managers help select the high priority energy topic areas that are to be emphasized in the URI program announcement. The annual awards are based on merit, the accomplishments of current DOE-supported research projects, and the degree to which the new equipment will enable researchers to substantially advance the understanding of energy-related phenomena.

The FY 1990 budget request of $5.0 million for the URI program will assist universities in purchasing instruments that cost over $100,000 each and that will be used by a number of researchers, including students. In selecting approximately 20 proposals to be funded, special emphasis will continue to be given to the need for innovative instrumentation needed to carry out research on advanced topics in energy research.

MULTIPROGRAM ENERGY LABORATORIES-FACILITIES SUPPORT

The goal of the Multiprogram Energy Laboratories-Facilities Support (MEL-FS) program is to reduce facility deficiencies and maintain safe, cost-effective, environmentally clean and productive operations at the five national laboratories overseen by the Office of Energy Research. To accomplish this, the MEL-FS program supports activities that counter problems of aging and obsolescence of facilities and of environmental compliance.

The MEL-FS program consists of two subprograms. The General Purpose Facilities Construction subprogram originated in FY 1981 as a broad program for rehabilitation, upgrade, or replacement of deficient buildings, utilities, roads, railroads and other related facilities at the laboratories. The Environmental Compliance subprogram originated in FY 1985 to address environmental deficiencies at Oak Ridge National Laboratory.

The FY 1990 budget request for MEL-FS is $53.0 million (Table 12). Of this amount, $32.5 million is for the General Purpose Facilities Construction subprogram. This request will provide for 26 construction projects, of which many are environmental, safety and health related improvements.

The FY 1990 request for the Oak Ridge National Laboratory Environmental Compliance subprogram is $20.5 million. Of the total request, $9.0 million is for Operating Expenses and $11.5 million is for Construction. The request for Operating Expenses is necessary to continue to characterize and investigate current waste handling and environmental control systems, leading to upgrades of these systems, and to continue remedial investigations and feasibility

Table 12
Energy Supply R&D
Multiprogram Energy Laboratories - Facilities Support
Budget Authority
($ in millions)

	FY 1988	FY 1989	FY 1990 Request
General Purpose Facilities			
Construction	$ 28.5	$ 31.2	$ 32.5
Environmental Compliance-ORNL			
Operating Expenses	12.9	8.8	9.0
Capital Equipment	.4	.3	0
Construction	11.8	15.4	11.5
Total Environmental Compliance	$ 25.1	$ 24.5	$ 20.5
Total Multiprogram Energy Laboratories-Facilities Support	$ 53.6	$ 55.7	$ 53.0

studies on previously contaminated sites. The Construction request will provide for continuation of a high priority project to upgrade a portion of the liquid low-level waste collection and transfer system at ORNL and initiation of a few minor general plant projects designed to correct deficiencies in operating systems. This subprogram is focused on bringing operational systems up to current environmental standards.

ADVISORY AND OVERSIGHT PROGRAM DIRECTION AND POLICY AND MANAGEMENT

The FY 1990 budget request for Advisory and Oversight Program Direction is $3.4 million. The FY 1990 request will provide the staffing resources required by the Director of Energy Research to carry out his responsibilities under legislation (P.L. 95-91) and those assigned or delegated by the Secretary in areas beyond the scope of other ongoing Energy Research programs. These funds are required to provide for the salaries, benefits, travel and other expenses associated with 44 full-time equivalent employees.

The FY 1990 request for Policy and Management is $0.7 million. These funds are required to provide for the salaries and related expenses associated with 8 full-time equivalent employees in the Office of the Director of Energy Research.

That concludes my statement. I would be happy to answer questions.

Senator FORD. Thank you very much, Dr. Hunter. Dr. Schwitters.

STATEMENT OF DR. ROY F. SCHWITTERS, DIRECTOR, SSC LABORATORY

Dr. SCHWITTERS. Mr. Chairman, Senator Bumpers, I want to thank you for this opportunity to appear on behalf of the many U.S. high-energy physicists who are deeply committed to building the Superconducting Super Collider as the next major instrument to further this exciting and historically important area of science.

The scientific case for the SSC continues to strengthen as questions about our present understanding grow, and as experience with today's experimental tools accumulates.

Today, I would like to present to you a few of my personal views as a person just entering this tremendous project. I have also submitted written testimony which gives a more detailed snapshot of the project, which has been prepared with the aid of experts from the central design group and new members of my SSC staff.

Recently, as you pointed out, Mr. Chairman, two major milestones for the SSC were passed. Ellis County, Texas, was selected to be the site and the university and industry team headed by the university's research association was chosen as the management and operations contractor. Thus, we now know the performers who are actually going to carry out the work on this project. In addition, these two events involve substantive review of the original cost estimates for the project, and in these reviews no evidence was uncovered of significant increases.

Now, I would like to make a few remarks about the site selection. I was actually a member of the National Academy panel that read all of the state proposals and prepared the best-qualified list for the Department of Energy. In reading every proposal, I was very impressed by their consistently high quality and I was deeply moved by the spirit, the genuine interest and boldness that many states brought to this project. Any one of those best qualified sites would have provided a successful home for the SSC. Texas was picked by DOE and it offers the country an outstanding site for the SSC. As director, however, I am committed to building a National Laboratory in Texas where the scientific, educational, technological and cultural benefits are available to all.

The selection of URA as SSC contractor means that the highly successful tradition of academic leadership of our national high-energy laboratories will continue. However, to help maintain a viable high-energy physics program during SSC construction to allow our scientists to focus on the demanding physics challenges of the SSC and to draw on outside experience with large technical projects, the URA-SSC team is establishing new working relationships with industry.

For example, URA has teamed with two national companies, EG&G and Sverdrup, in the management of the laboratory. Sixteen major industrial concerns have joined the magnet industrialization program, giving them first-hand experience in the R&D program on the magnets.

Just two weeks ago the first International Industrial Symposium on the Super Collider was held in New Orleans. It was a smashing success, with over 700 participants from industry, from the academic and scientific community and from the citizens of Ellis County, Texas. We in the SSC management seek industrial involvement wherever possible to further this project.

Many challenges face our new SSC management team, but we also have an opportunity now to take a fresh look at the project. The previous central design group work has provided us with a very strong base from which to work, but over the next year the new group, including many of the old members, by the way, will be questioning everything as we move to a definitive design for the project. We are not going to make changes without compelling reasons, and we are committed to use our best efforts to keep the construction budget within the present cost estimate.

I would like to just outline for you some of the high priority tasks that I see ahead of our group. First and most important, we have to begin recruiting the first-rate staff to carry out this big project. We have to prepare a site specific design and cost estimate. We have to pursue aggressively to completion the necessary technical R&D for the accelerator systems.

The major effort here will continue to be the magnet program, which has successfully produced and tested two full-scale and several—in fact, many—shorter prototype magnets. This is an area of particular concern to all of us, and I have appointed an expert review panel to fully examine that magnet program and make sure that it is what we need for the project.

The fourth item on my list is to begin the process of initiating the scientific program whereby we will continue to foster Nationwide detector R&D and establish guidelines and schedules to begin the selection of the experiments. This activity, I should point out, involves major participation throughout the universities throughout the country.

The fifth point is to work with government to secure the nonfederal support, sources of support, that we have talked about earlier today.

The sixth point that I would like to mention is to establish a base of operations near the site so that we can focus our energies there.

Now, of vital importance to all of these tasks on our agenda is a need for a commitment now by the U.S. government that the SSC will go forward.

We are pleased with the President's DOE budget request for Fiscal Year 1990 funding. Congress' adoption of this funding level will provide the commitments required to initiate this project with the vitality needed to complete it in a timely and cost-effective way.

I personally am honored to have been selected to direct the SSC. It is a unique privilege to have the opportunity to create from this really lovely countryside of Ellis County a great international laboratory where some of the world's best scientists and their students will flock to work on the greatest scientific instrument of our time.

I look forward to working with you on this project. I will certainly do everything I can to keep the Subcommittee fully informed on all of our activities. Thank you for your consideration, and I would be happy to answer of your questions.

[The prepared statement of Dr. Schwitters follows:]

SSC Status

Prepared testimony of Prof. Roy Schwitters, Director

Superconducting Super Collider

before

Subcommittee on Energy, Research, and Development

Committee on Energy and Natural Resources

United States Senate

February 24, 1989

Introduction

Mr. Chairman, Members of the Committee, it is a real pleasure to appear before you today to testify about the Superconducting Super Collider. As the new Director of the SSC Laboratory, I represent the consensus of the whole U.S. high-energy physics community who have, for several years now, supported the idea of a speedy construction of this scientific instrument as the number one priority of the U.S. particle physics program.

These are exciting times for high-energy physics. We feel that our ability to achieve energy levels that will lie in the domain of the SSC will allow us to answer some of the questions that mankind has investigated for several millenia. In my testimony, I hope to indicate some of the scientific motivations for this instrument, describe the progress that has been achieved on the SSC during the last few years, and outline to you our plans for the future.

Particle Physics Today

The ultimate secrets of matter and energy are to be found in the world of elementary particles. These particles make up all matter, and they obey laws that, as far as we know, apply at any time and place in the universe. The goal of particle physics is to map out this world and discover the rules that govern its behavior. This knowledge has far-reaching consequences for our understanding of what the universe is and how it came to be.

Ours is hardly the first time in history that physicists have believed themselves close to an enduring understanding of the laws of nature. What, then, are the grounds for believing that, whether or not a grand synthesis of natural law is near at hand, important progress is under way? Over the past two decades, the age-old struggle to describe and comprehend the nature of matter has been rewarded by a radically new and simple picture of nature on the most basic level. A host of new experimental results, made accessible by a new generation of particle accelerators, and the accompanying rapid convergence of theoretical ideas have brought to the subject a new coherence and have raised new aspirations. The new insights not only provide a framework for describing and understanding the world around us, but also elucidate the first instants after the creation of the universe.

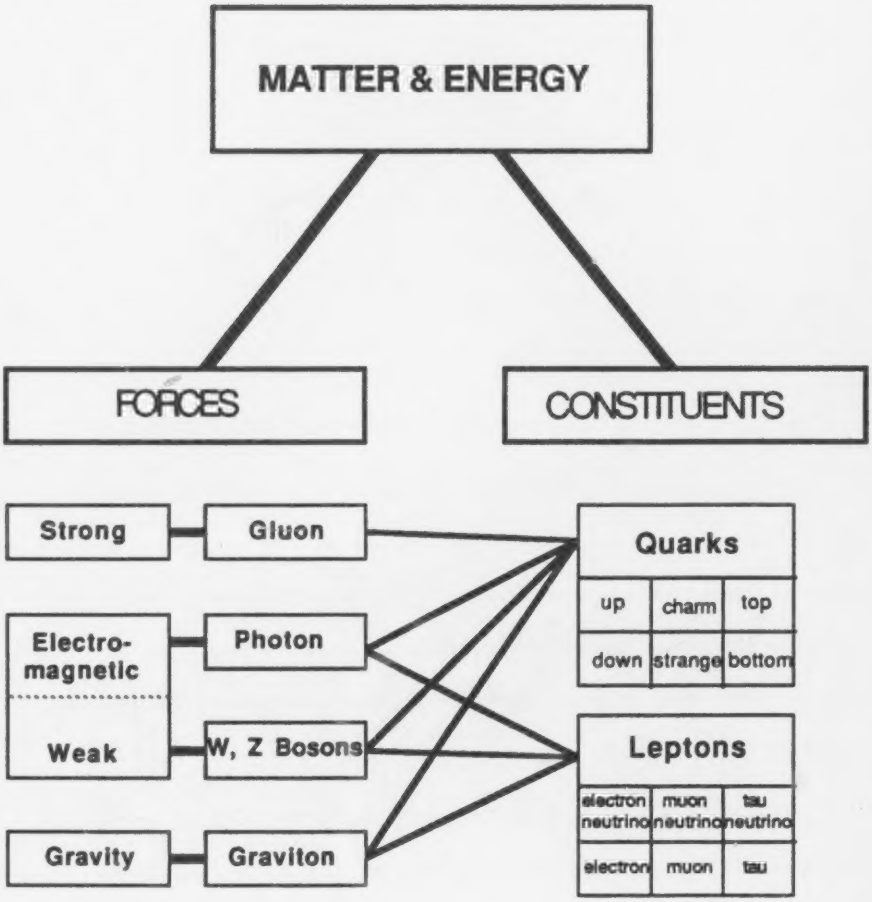
Our present picture of the composition of matter has two elements: a set of fundamental constituents and a small number of fundamental forces, or interactions. These are represented in the accompanying figure. The fundamental constituents are called *leptons* and *quarks*. A class of mathematical theories called *gauge theories* provides a common framework for discussing all the fundamental interactions and therefore gives us the hope of seeing connections between these interactions.

Some Open Questions

The advances of the past decade have brought us tantalizingly close to a profound new conception of the basic constituents of matter and their interactions. The simpler and more comprehensive understanding we have gained organizes current knowledge and locates the horizon of particle physics at energies of trillions of electron volts and the horizon of cosmology at about a millionth of a billionth of a second after the moment of the creation of the universe as we know it as physicists. Even though lower energies may provide hints of new phenomena, exploration of these uncharted territories lying beyond the horizons of present knowledge is bound to yield new insights into the unknown.

The understanding that we have gained during the past year into the world of fundamental constituents is a great achievement, on a par with the discovery of the periodic table and the beginnings of the invention of quantum mechanics. It shows the way toward a simple, organized explanation and understanding of everything that goes on in the universe at the most elementary level. However, as our conception of matter based on quarks and leptons

THE STANDARD MODEL


MATTER & ENERGY
FORCES
CONSTITUENTS
Strong
Gluon
Electro-magnetic
Photon
Weak
W, Z Bosons
Gravity
Graviton
Quarks
up
charm
top
down
strange
bottom
Leptons
electron neutrino
muon neutrino
tau neutrino
electron
muon
tau

has become better established, we have become increasingly aware of its shortcomings. Having arrived at a new level of understanding, we can look critically at our collective achievement and try to ask what we haven't succeeded in doing.

Since the late 1970s, the international scientific community has recognized that our new understanding is incomplete. We can define a frontier where the standard model ceases to make sense. The response sets the scale of the Supercollider: It is the scale of energies of about *one trillion electron volts* for the collisions between quarks and leptons and the force carriers. Or, if you like, it is on a very small distance scale: distances less than a billionth of a billionth of a meter. This is the place where our current understanding doesn't quite make sense, where there must be something new and interesting going on.

The questions about nature that are raised, but not answered, by our current model indicate the kind of initial exploration that we want to conduct with the Supercollider:

We want to know what determines the properties of the quarks and leptons, just as our scientific ancestors asked themselves what caused the elements to conform to certain patterns of behavior. We can arrange the known quarks and leptons in the little boxes shown in the figure and notice the relations between them, but how much more satisfying it would be if we could tell how much a quark or lepton will weigh and how it will behave under particular circumstances.

We have evidence for six quarks and six leptons, but we don't have any reason why there should only be six of each, or that there should indeed be six of each. How many of them are there? From a self-centered perspective, just having only two quarks and electrons would seem to be fine: that is the stuff we are made of. What role do the other quarks and leptons play in shaping the world around us?

There are many close similarities among the quarks and leptons. Indeed, mathematical consistency of the theory that brings radioactivity and electromagnetism together requires that there be a connection between quarks and leptons. For each set of quarks there has to be a set of leptons. How are the properties of the quarks and the leptons related?

All the direct experimental evidence so far is consistent with the quarks and leptons being fundamental. We haven't seen any structure inside them, but we recall that atoms, nuclei, and protons all looked fundamental at the time of their discovery. Yet, when they were scrutinized more closely, there was something inside, there were simpler constituents to be found. Are there even simpler and more fundamental things to be found inside the quarks and leptons? The observation of constituents within the quarks and leptons would give us a better understanding of the properties of today's fundamental constituents and the ability to construct a full picture of nature.

We may ask whether there are other kinds of matter, not made of quarks and leptons, that we haven't yet seen.

In addition to those questions that pertain to the constituents, there are some questions that have to do with the interactions. We have found a nearly complete connection between the weak interaction and electromagnetism. However, there is a fundamental difference between the interactions that we observe. Electromagnetism acts over very large—astronomical—distances. We can check the laws of electromagnetism out to interplanetary separations, and the influence of electromagnetism is felt at those vast distances. The weak interaction, which is friend and partner to electromagnetism, acts only over a range which is small even compared with the size of an atomic nucleus. What makes radioactivity and electromagnetism different and yet at the same time so similar?

Having seen the complete unification of electricity and magnetism in electromagnetism, and the partial unification of the weak and electromagnetic interactions, we may ask whether there is in fact an underlying unity among all the fundamental forces. We may also inquire whether there are new fundamental forces that we haven't seen before.

These questions—together with our appreciation of the countless instances when unexpected, puzzling, and ultimately illuminating new observations have come out of accelerator experiments—motivate our quest for a microscope capable of looking at even smaller distances: an accelerator of higher energies.

Physicists do not believe that within the energy range now available there are or will be enough clues for us to piece together a theory of the nature of matter. Both our specific questions and more general arguments set a scale of energy or size that we need to explore, the domain of energies of about a million million electron volts, of distances smaller than a billionth of a billionth of a meter. The instrument of choice for opening this frontier is one of the most extraordinary scientific instruments ever conceived: the Superconducting Super Collider.

The SSC

The Superconducting Super Collider (SSC) will be a chain of accelerators, linked to boost barely moving protons to nearly the speed of light. The particles reach increasingly high energies in a cascade of four accelerators that make up the injector, from which they are fed into the collider ring.

The collider ring will be the heart of the SSC. In this underground oval tunnel, ten feet in cross section and fifty-three miles in circumference, two beams of protons will be accelerated in opposite directions. Superconducting electromagnets in the collider ring will guide each proton beam through a stainless steel pipe about one and a quarter inches in diameter. Magnets will curve the paths of the beams, making it possible to build the accelerator in a loop, rather than a straight line, and thus realize substantial economies by repeatedly using the same components to accelerate and contain the beam. The protons will travel in compact bunches, with some eight billion protons in each bunch. Each second, the fifteen thousand bunches that will fill each beam pipe will make three thousand laps around the ring. On every lap, a pulse of radio waves will boost the energy of each bunch, until the protons reach their final energy—twenty trillion electron volts. The two circulating beams of protons will be maintained at that energy and focused into head-on collisions where detectors will record particles produced from the energy of the collision.

Superconducting magnets will be used because they make possible substantial cost savings, both in construction and in operation of the SSC. At low temperatures, certain materials become superconductors. A superconducting material dissipates no energy in heat—it just carries the current you put into it *without electrical resistance*. Such a material, an alloy of niobium and titanium, will be used to fabricate the coils for the SSC magnets. The coils are made of copper wires, inside of which are embedded thousands of fine fibers of niobium-titanium alloy, each about one-sixth the diameter of a human hair. It is those niobium-titanium fibers that carry the current. When cooled to the temperature of liquid helium, about four degrees Kelvin above absolute zero, each one of the strands is capable of carrying about five hundred amperes. That is twenty or thirty times what a slightly bigger wire carries in a house before the fuse blows. Once the SSC magnets are turned on, electric power is only required to run the refrigerators that keep them cold, not to replenish the current in the magnet coils. A non-superconducting supercollider with conventional magnets would require four gigawatts of electrical power, resulting in an electric bill of about one billion dollars per year. The SSC's average power requirements are about forty times smaller. In addition, superconducting magnets can create magnetic fields several times stronger than conventional magnets, thus confining the protons to a tighter orbit and reducing the ring's circumference.

At the interaction regions the two beams will be steered into head-on collision. The protons' vast energy, concentrated in a minute volume, will materialize in a brief flash of new particles. International teams of scientists will design and operate experimental apparatus to identify the particles produced and to determine as precisely as possible the path each particle follows. Although similar in concept to existing detectors, SSC detectors will be larger and more complex: a typical detector will weigh about 50,000 tons and will require a million channels of electronics to filter out the interesting events from the 100 million collisions that will occur every second.

Although many of the phenomena that will be revealed do not occur at the relatively low energies of ordinary conditions on Earth, they play a crucial role in the unification of forces and in the dynamics of the very early universe. If the history is any guide, this deeper understanding that will allow us to codify the basic laws of nature will eventually result in putting that knowledge to humane and productive use.

The Connections of Elementary Particle Physics with Other Sciences

Elementary particle physics does not exist in isolation, either as a scientific discipline or as a technological enterprise. Stimulation, information, and techniques flow both ways: from other activities toward particle physics, and from particle physics toward other activities.

The intellectual links to other sciences are strong. They grow more robust as our own field of elementary particle physics develops a greater unity and coherence. Quantum chromodynamics, a theory of the strong interaction between quarks, finds applications and some of its most probing tests in nuclear physics. We share with our colleagues in atomic, molecular, and optical physics an interest in tests of quantum electrodynamics and measurements of the fundamental properties of the electron, muon, and other basic particles. In the last decade, a close working relationship has developed between elementary particle physicists and astrophysicists and cosmologists concerned with the evolution of the universe. The laws of nature uncovered in the study of particle physics are essential ingredients in imagining what the early universe must have been like. Astrophysical relics from those early moments when the universe was small, hot, and dense test our ideas about the fundamental interactions at higher energies than we will ever be able to attain with accelerators.

Much of our insight into the nature of complex systems built up from simple components and simple rules comes from the science of condensed-matter physics. We share with this field many theoretical tools, and an interest in hidden symmetries and changes of phase, or state. Traditional connections between theoretical particle physics and mathematics are once again in great ferment, with the recognition of the common ground shared by fibre bundles and the gauge theories of the fundamental interactions, with the rich interchange between new mathematics and superstring theories, and with the emergence of the interdisciplinary study of complex (nonlinear) systems called "chaos."

The health of these other fields of basic research is essential to the vitality of elementary particle physics and the advance of basic research requires vigorous progress in particle physics.

Particle physics also enjoys strong links to new technologies. A few examples will serve to illustrate how benefits flow both to and from elementary particle physics. Synchrotron light sources, the offspring of accelerator science, now find routine application to materials research, biomedical studies, and the fabrication of electronic components. The high-density semiconductor devices manufactured in this way are important elements of the detectors used in elementary particle physics experiments. Advances in the capability of the niobium-titanium superconducting cable so essential to the Supercollider (and to the availability of magnetic resonance imaging devices for medical diagnosis) have resulted from a purposeful collaboration among accelerator builders, university-based metallurgists, and industrial firms. The demands of particle physics have spurred the development of fast electronics and new computer architectures to handle high data rates, and to make the rapid decisions required by modern detectors. Theorists, experimenters, and accelerator builders are quick to take advantage of new capabilities spawned elsewhere in networking, the application of remote local intelligence to process control and engineering design. The components of our detectors, including semiconductor devices, tracking chambers, and calorimeters, also provide fruitful interchange with other researchers, manufacturers, and developers of technology in fields such as medical imagery.

A Brief History of the SSC

Although the scientific need for a step to much higher energies was already apparent, in broad terms, a decade ago, it was necessary to develop the technical means to take that step. Formal studies of the feasibility of a multi-TeV accelerator began with workshops sponsored by the International Committee on Future Accelerators at Fermilab in 1978 and at CERN in 1979. Various possibilities for very high energy accelerators, including proton-proton colliders at tens of TeV per beam, were examined. The idea of the SSC began to take shape at the 1982 Summer Study on Elementary particle Physics and Future Facilities organized in Snowmass, Colorado, by the Division of Particles and Fields of the American Physical Society. The SSC initiative was followed up with workshops on accelerator and detector issues held during 1983 at Cornell University and at the Lawrence Berkeley Laboratory.

Encouraged by the successful operation of the Fermilab Tevatron (the first superconducting synchrotron), where protons have been boosted to energies of nearly one trillion electron volts, a High Energy Physics Advisory Panel (HEPAP) Subpanel on Future Facilities recommended "the immediate initiation of a multi-TeV, high-luminosity [high collision-rate] proton-proton collider project with the goal of physics at the earliest possible date." This recommendation was transmitted to the Department of Energy with the unanimous endorsement of HEPAP in July of 1983. The DOE responded by reprogramming, with Congressional approval, of funds for preliminary R&D for the SSC in the fall of 1983. Since that time, on two separate occasions—once in 1985 and once in 1989—comprehensive reviews by HEPAP of the U.S. high energy physics program reaffirmed the timely construction of the SSC as the number-one priority in that field.

In December 1983, the DOE and the Directors of the United States high-energy physics laboratories chartered a Reference Designs Study. The goal was to produce example designs of a proton-proton collider that would yield about 100 million collisions per second with energy of 20 TeV per beam. Three different approaches were investigated, and it was found that any of them could form the basis of a technically feasible SSC, at a cost of approximately three billion (FY 1984) dollars.

Early in 1984, the Department of Energy contracted with Universities Research Association, a consortium of leading research universities, to direct and coordinate the SSC research and development program. For this purpose the SSC Central Design Group (CDG) was established, accepted the hospitality of the Lawrence Berkeley Laboratory, and began work on a non-site-specific conceptual design. Other participants in this national effort included Brookhaven National Laboratory, the Fermi National Accelerator Laboratory (Fermilab), Lawrence Berkeley Laboratory, the Texas Accelerator Center, and experts from many laboratories and universities in Canada, Western Europe, Japan, and the United States. American industrial firms also played an important role.

Since approximately 8000 dipole magnets will generate the magnetic field that keeps the protons circulating around the SSC collider rings, an early goal was to determine the basic dipole magnet style. In September 1985, after several different concepts had been developed and studied, a high-field superconducting magnet, in which the field is defined by the placement of the superconductor, was adopted as the basis for the conceptual design.

Simultaneously, all accelerator and conventional systems (other than detectors) were defined and assessed. The completed Conceptual Design Report (CDR) details the scientific, technical, and cost aspects of the project. Since its publication in March 1986, the Conceptual Design Report (SSC-SR-2020) has been extensively reviewed by independent experts. The reviews of this document formed the basis for the decision in January 1987 by President Reagan to endorse construction of the SSC as a Presidential initiative. Subsequently, the Conceptual Design Report has served as the basis for continuing SSC R&D.

Selection of a Site and a Management and Operations Contractor

The Department of Energy initiated the SSC site selection process in April, 1987, by issuing an invitation for site proposals which described the facility, the site selection criteria, and the selection process to be followed. DOE asked the National Academy of Sciences (NAS) and the National Academy of Engineering (NAE) to form an evaluation committee which would review the qualifying proposals and identify several best qualified ones.

Forty-three site proposals were submitted and thirty-six of these were judged by DOE to satisfy their qualifying criteria. Those proposals were forwarded to the NAS/NAE committee for evaluation and in December, 1987, that committee submitted a report identifying eight of the sites as the best qualified ones (one of these eight was subsequently withdrawn by the proponents). The DOE concurred with the recommendation of the committee and initiated detailed evaluation of the seven remaining sites.

As a result of this evaluation process, the Texas site, located in Ellis County, was designated the preferred SSC site by the Secretary of DOE in November, 1988. Pursuant to subsequent study of the environmental impact, the preferred site was designated the SSC site on January 18, 1989.

In August, 1988 the DOE issued a call for proposals for management of the construction and, as an option, of the subsequent operation of the SSC Laboratory. Experience in high energy physics activities and in construction of large-scale projects were deemed the most important criteria in the selection process. The Universities Research Association, a consortium of then sixty-six leading research universities, submitted a joint proposal together with two industrial companies, EG&G and Sverdrup. This proposal was accepted and the URA team was designated as the SSC M&O contractor on January 19, 1989.

Priorities for the Next Eighteen Months

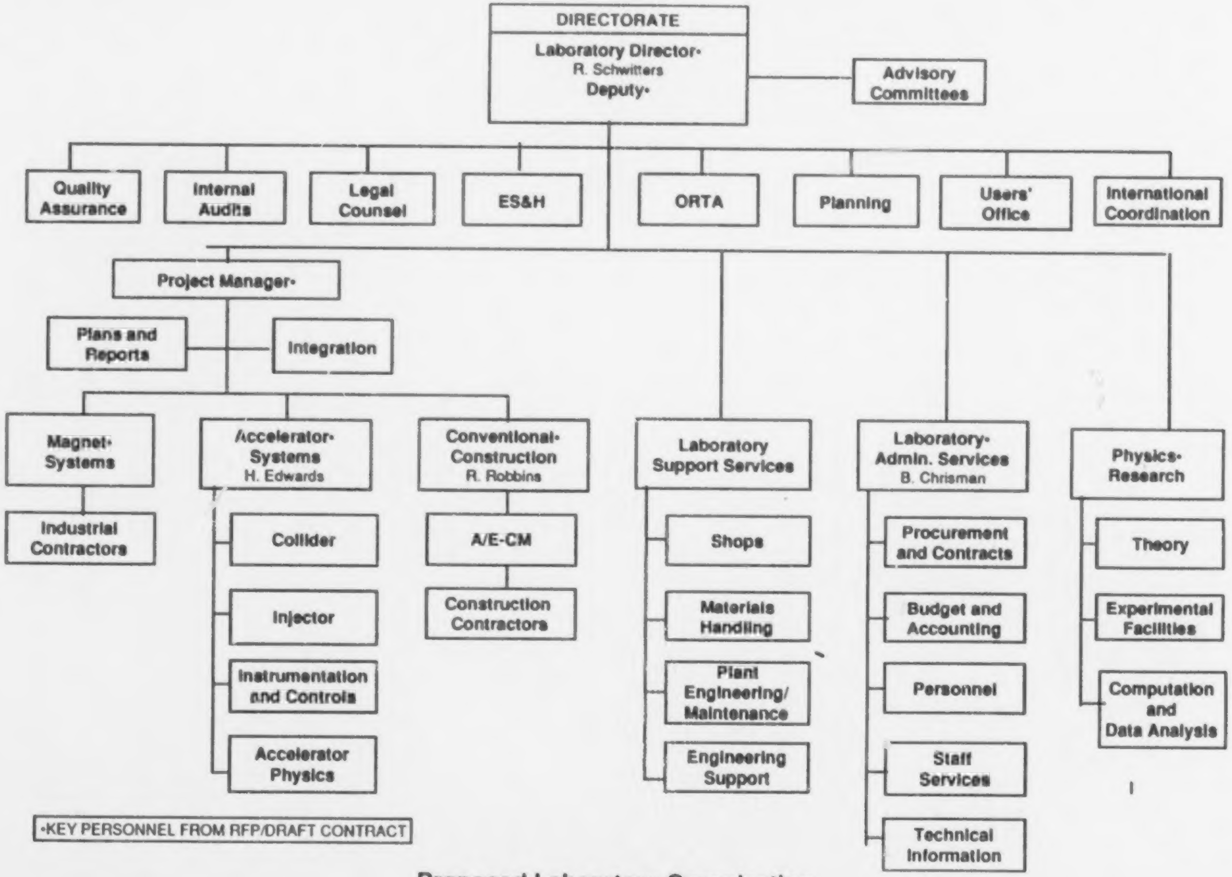
The selection of the SSC site in Texas and of the M&O contractor, URA, allow us now to proceed with the tasks that are necessary to build a new high energy physics laboratory. In embarking upon these tasks we are fortunate that we are able to start from the foundations established by the four years of R&D work performed under the leadership of the SSC Central Design Group.

We anticipate that many of the senior people from the CDG will form the core of the leadership at the new SSC laboratory and we hope that the large majority of its dedicated staff will join the new SSC organization. The continuation of the R&D and the initiation of the construction phase will, of course, require a significant growth in the staffing level; we are beginning to embark on the recruiting task, hoping to find and hire the best people in the world for the key positions at the laboratory. Our goal is to build up, over time, a technical staff that will operate the laboratory once it is completed. To fill the personnel "bulge" that will occur during construction, we plan to rely on our two industrial partners, Sverdrup and EG&G. Industrial involvement will be critical to the efficient construction of the laboratory. It is our intention to subcontract with private industry for a number of manufacturing and construction activities and, in the operational phase, for various specific tasks required at the laboratory.

The next several months will be a period of transition, both geographic—from Berkeley, California, to Ellis County, Texas—and organizational—from the SSC Central Design Group, the organization that carried out the pre-construction R&D mission, to the SSC Laboratory. We are currently in the process of defining the new SSC organizational structure, and our current thoughts on this subject are presented in the attached organization chart.

We have already obtained limited office space not far from the site that will become the nucleus of the activities associated with the new laboratory. A very important part of our transition activities is the re-evaluation, in light of the recent SSC developments, of the budget priorities during FY89 consistent with the Congressionally mandated prohibition of construction activities..

The Conceptual Design Report is still the most authoritative source on the design and the cost of the Supercollider. Since its completion, several important new developments have occurred which might have some impact on the design and cost of the facility. Foremost among them is the selection of the site, which allows us now to go ahead with the preparation of the site-specific conceptual design. The research and development work conducted during the last three years, both theoretical and experimental, has yielded new information that will result


DIRECTORATE
Laboratory Director•
R. Schwitters
Deputy•
Advisory Committees
Quality Assurance
Internal Audits
Legal Counsel
ES&H
ORTA
Planning
Users' Office
International Coordination
Project Manager•
Plans and Reports
Integration
Magnet• Systems
Industrial Contractors
Accelerator• Systems
H. Edwards
Collider
Injector
Instrumentation and Controls
Accelerator Physics
Conventional• Construction
R. Robbins
A/E-CM
Construction Contractors
Laboratory Support Services
Shops
Materials Handling
Plant Engineering/ Maintenance
Engineering Support
Laboratory• Admin. Services
B. Chrisman
Procurement and Contracts
Budget and Accounting
Personnel
Staff Services
Technical Information
Physics• Research
Theory
Experimental Facilities
Computation and Data Analysis

•KEY PERSONNEL FROM RFP/DRAFT CONTRACT

Proposed Laboratory Organization

in modifications and improvements of many technical components of the facility. Finally, the vigorous effort that has been extended worldwide in various workshops on physics opportunities at the high energy colliders has resulted in a better understanding of the requirements for the experimental halls where the detection apparatus will be located. This information will allow us to define more precisely the design of the experimental areas.

The cost estimates given in the Conceptual Design Report stand as an accurate representation of the total cost of the SSC facility. It is prudent and customary to reexamine the total estimated cost of any construction project on an annual basis. Accordingly, we plan to obtain an updated, bottoms-up cost estimate of the SSC, using the new information available, and we hope to have this completed during the next year. This cost estimate will be performed in parallel with an extensive review of the whole project by the Director. Our intent is to assure that the SSC Laboratory can be built, and then operated, efficiently and on time, within the budget limits now specified.

The research and development effort will continue to have maximum priority and will build on the work initiated by the Central Design Group. Development of optimum and cost-effective superconducting magnets will be pursued vigorously. Any R&D program sorts out competing designs, identifies and corrects flaws in design and process, and prepares for transition to final manufacture in the most efficient manner. Even though the superconducting magnet R&D program for the SSC has encountered many complex and difficult problems, most of those have already been solved and, as of today, two full-length magnets meeting SSC performance specifications have been produced. The work that has been carried on heretofore at the laboratories will be augmented by the efforts of private industry within the framework of the magnet industrialization program, a program that is now under way and that has been characterized by active and enthusiastic participation by U.S. and world industry.

In parallel we shall pursue technical design of the injector system, continue our studies of beam dynamics in the SSC, enlarge the scope of work on cryogenics and other accelerator systems, and increase our efforts on the SSC detector systems. This latter effort will be carried on principally at universities and laboratories all over this country, and abroad as well. The R&D goals for FY90 are discussed in more detail in another section of this presentation.

The purpose of the SSC is to investigate the fundamental laws of nature, and thus the organization and initiation of the scientific program will be a very high priority during the next eighteen months. The SSC will be a leading instrument in the field of high energy physics and a major focus of interest not only of a majority of the U.S. particle physics community, but also of a significant fraction of scientists abroad. To assure the highest quality scientific program at the SSC, I plan to appoint soon an International Scientific Policy Committee to advise me on formulation of the guidelines and time scales for the initiation of the physics research work at the SSC. We shall continue the CDG tradition of sponsoring workshops on both technology and physics that are relevant to the SSC, and anticipate wide involvement of the whole U.S. high energy physics community in these efforts. At a somewhat later date, I plan to appoint a Program Advisory Committee, with a function of providing advice about specific aspects of the experimental program.

It is the stated intention of the U.S. government to seek foreign participation in the construction of the SSC. The establishment of the SSC Laboratory administration and the demonstration of a commitment to move ahead with the laboratory will now allow us to take concrete measures to achieve that goal. It is our intention to embark immediately on high-level contacts with our senior colleagues abroad as well as with science administrators in foreign countries with the objective of defining clearly their interest and our own, and examining their potential to provide specific systems and subsystems for the SSC. These contacts will build on demonstrated extensive interest from scientists abroad in utilization of the SSC that has exhibited itself in extensive foreign participation in various technical and scientific workshops. The appropriate administrative conditions are now being put in place to allow us to initiate efforts to translate these interests into specific commitments to participate in SSC construction. We feel that U.S. commitment to this project is essential if we are to be successful in our efforts with our foreign colleagues.

Budget Summary

With the selection of the site for construction of the SSC, the efforts of the CDG design team are about to be realized. URA, as the M&O contractor, is prepared to continue the design work for the SSC and looks forward to the eventual operation of the research facility on behalf of the Department of Energy for the benefit of the country and the high energy physics community.

The budget request presented here outlines funding for research and development and for construction. Considerable research and development attention is directed toward the design, production, and testing of long and short superconducting magnets. Brookhaven National Laboratory will continue to produce prototype yoked magnet coils for insertion into helium cryostats at Fermilab. (BNL will also have its own limited capability for cryostating and testing magnets.) The superconducting cable developments will be centered in industry with supervision and coordination by the Lawrence Berkeley Laboratory and SSC personnel. In addition, Fermilab will be expected to fabricate long and short magnets for testing in the magnet test facility.

Long lead-time procurement of material such as superconducting cable will be needed to set the stage for the subsequent steps of magnet coil winding by industry. R&D funds will be used in FY 1989 to acquire additional cable-making machines. These machines will be provided as government-supplied equipment to the successful industrial superconducting cable suppliers for the SSC production needs in 1990 and beyond.

The other portion of the budget request concerns the use of construction funds for the project, much of which will be directed toward the design of the technical components for the accelerator facilities and the start of long lead-time items. The main item is the continuation and expansion of the Magnet Industrialization Program. Following the technology orientation phase of the program a selected number of vendors will be invited to design and fabricate production-quality tooling prototypes for the pre-production of superconducting collider dipole magnets. Emphasis will naturally be placed upon the successful production of a substantial number of magnets—magnets that when cooled and tested meet the performance objectives.

Fiscal year 1990 represents the start of preliminary design efforts for numerous technical components for the SSC. The requirements for the superconducting quadrupoles must be refined so that pre-production units can be made. Design of cryogenic elements that connect the quadrupoles to the ring dipoles will be completed and this will permit completion of the preliminary design efforts for the overall collider ring cryogenic system. Other important design objectives are centered on the injector. Following determination of the energy of the proton beam to be injected into the collider, the final design of the high energy booster can proceed. An important related topic concerns the test beams to be provided from the injector. Early availability of those facilities will be crucial to the testing of components for the SSC detectors.

Progress with siting studies will allow precise determination of the location of the large collider ring on the Texas site. It is expected that the campus area will be identified and plans begun for the construction of laboratory facilities. One of the most important will be the Magnet Test Laboratory. This facility will be needed to prepare, cool down, and test the superconducting magnets produced by industrial suppliers. It will play an important role in qualifying magnet vendors as well as serving as a quality control facility in the future.

The development of site facilities will require the procurement of electrical transmission line and substation equipment. Such capabilities will be needed in the campus area for the Magnet Test Laboratory, and will be required for the commissioning of technical systems. Procurements need to be initiated for such long lead-time items as substation transformers, switchgear, towers, and cable for electrical distribution.

Finally, increased attention will be given to the preparation of detector systems for future experiments at the SSC. Universities, laboratories, and industries are expected to participate in the research and development activities on detectors. The testing of components, needed to support the design and construction of these detectors, will be done in the initial stages mainly at Fermilab, with some testing being carried out at other laboratories both in the U.S. and abroad.

SSC FY90 PROGRAM

(PRESIDENT'S BUDGET REQUEST)

		FY90 M$
Construction Program		160.0
• Technical Systems design	29.0	
• Conventional Systems design	29.0	
• Site preparation, utilities and support buildings	24.0	
• Technical Systems – Advanced procurement and magnet industrialization	50.0	
• Support equipment and rental space	10.0	
• Project Management	18.0	
R&D Program		90.0
• Accelerator component development	59.0	
• Detector Systems design and development	19.0	
• Accelerator development equipment	12.0	
TOTAL PROGRAM		250.0

The R&D Program

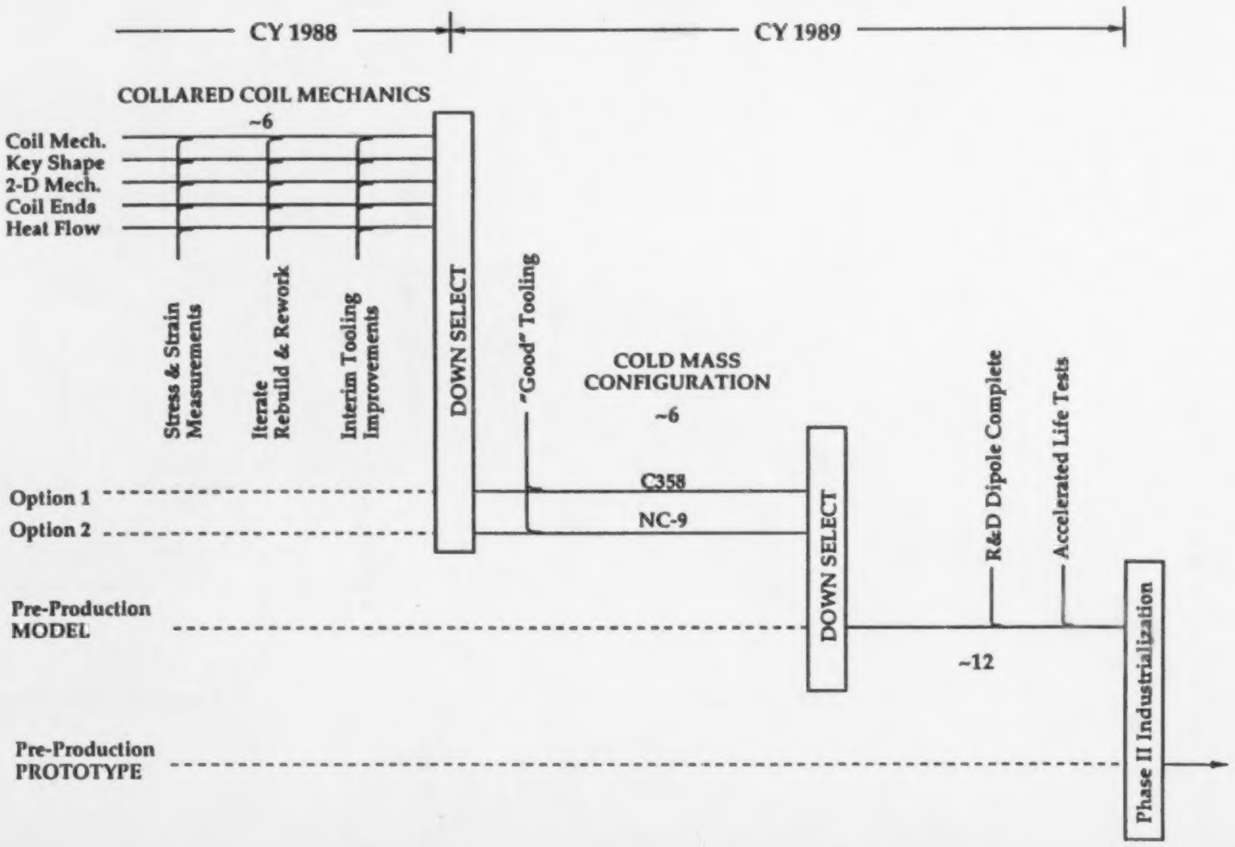
Superconducting Magnets. The principal goal and focus of the SSC magnet program in FY89 is the completion of the baseline design for the SSC collider dipole magnet. Both the superconductor and the magnet mechanical design aspects are reaching completion in that year with the fabrication and testing of six full-length baseline model dipoles and a comparable number of short magnet versions of the same design. Alternate R&D versions of the baseline design incorporating aluminum collars or axially-free collared coils were also tested in full-length development model magnets.

From this program, a magnet baseline design will be achieved and prototypes tested by the end of the fiscal year. The attached development logic diagram summarizes the decision process, and the attached figures show the performance of two successful full-length prototypes as well as the results of the test of a short magnet utilizing current design. The baseline model will meet all SSC systems requirements for the collider dipole application, and the resulting design will serve as an appropriate departure point for development in industry of a production version of the collider dipole and its associated production tooling. The FY89 magnet measurement and testing program will experimentally establish achievement of the needed magnetic field quality for service in the SSC collider as well as demonstrate the required mechanical, electrical, and thermal qualities to meet the SSC single magnet system needs and requirements. In addition, development of the collider quadrupole magnet design will be initiated in FY89.

It is the clearly stated policy of the SSC that the full complement of collider dipole magnets required for the project will be constructed in industry, and our Magnet Industrialization Program has been planned to carry out this task. The first phase of this three-phase program, the transfer of laboratory-originated technology to industry, is in progress and will be completed

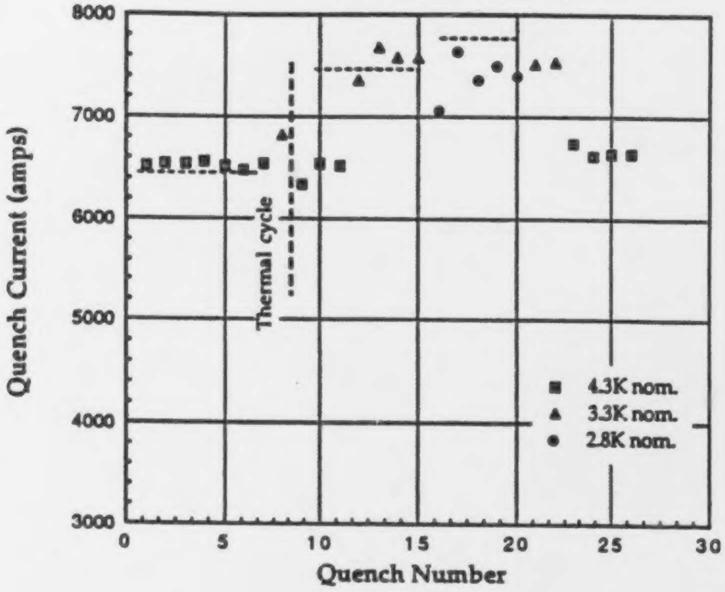
MT – 9/24/88

SSC Collider Dipole Development Logic

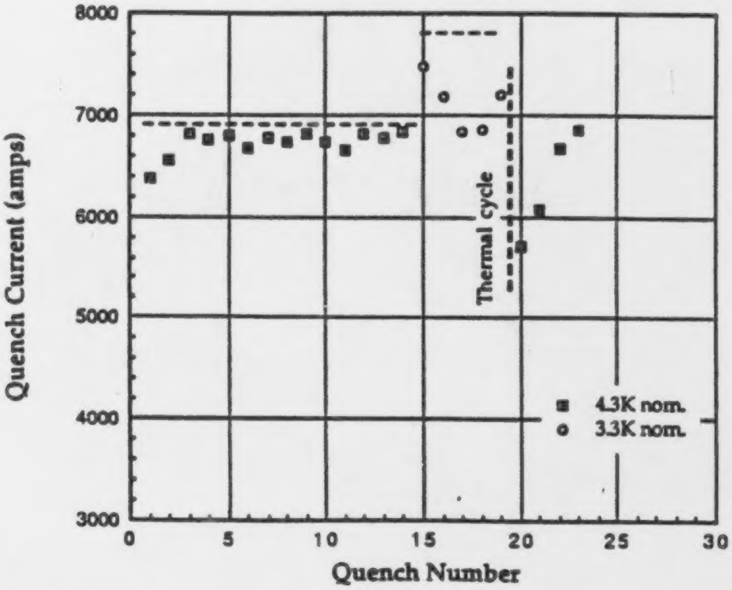

CY 1988
CY 1989
COLLARED COIL MECHANICS
~6
Coil Mech.
Key Shape
2-D Mech.
Coil Ends
Heat Flow
Stress & Strain Measurements
Iterate Rebuild & Rework
Interim Tooling Improvements
DOWN SELECT
"Good" Tooling
COLD MASS CONFIGURATION
~6
C358
NC-9
Option 1
Option 2
Pre-Production MODEL
DOWN SELECT
R&D Dipole Complete
Accelerated Life Tests
~12
Phase II Industrialization
Pre-Production PROTOTYPE

Recent Test Results
Full Length SSC Collider Dipoles

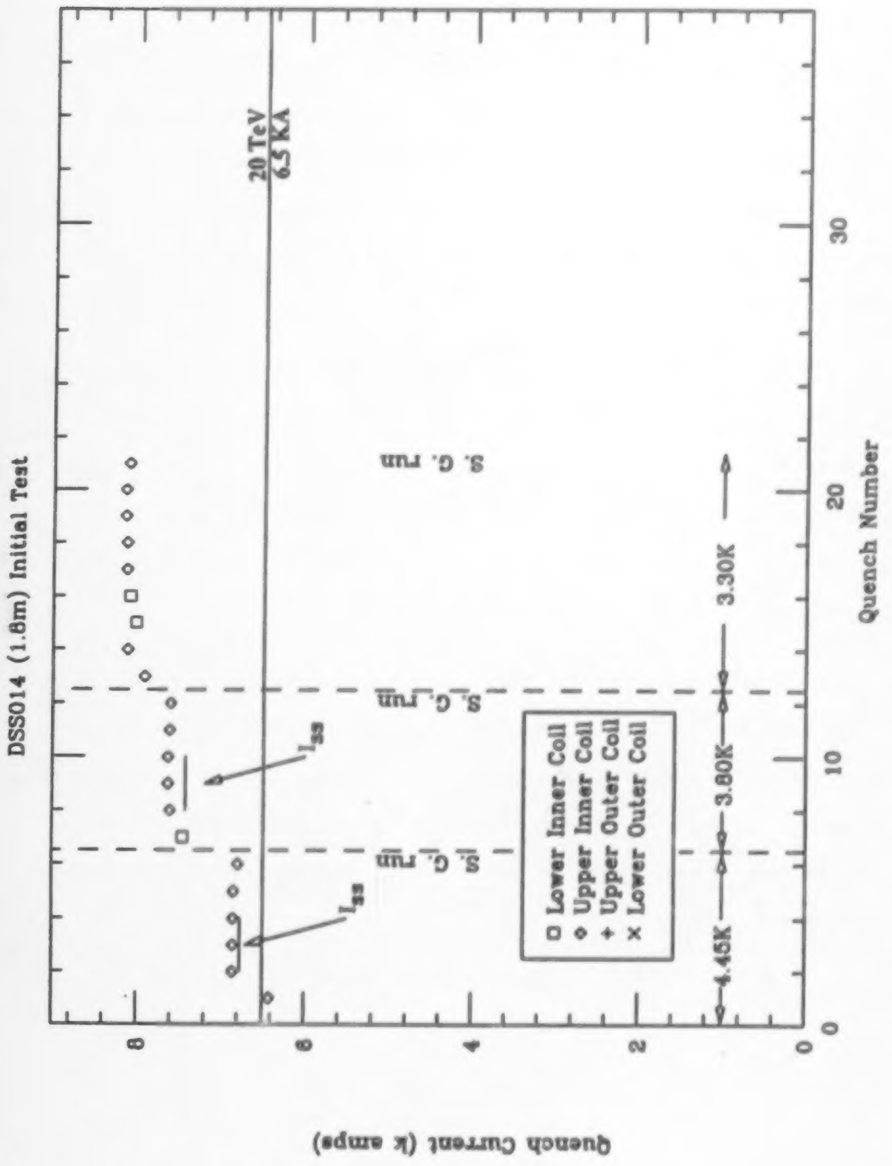
DD0012: Current at Quench



DD0014: Current at Quench



Current Design Version
SSC Collider Dipole - 1.8m Model
Quench Test Results, DSS014



by the end of February. The industrial response to this program has been very positive. We plan to issue a Request for Proposals for the industrial development of coil tooling and coils during this fiscal year. The currently anticipated timeline for the industrialization process leading to the industrial production of the superconducting dipoles is shown in the attached figure.

The emphasis for FY90 in the R&D program will turn to experimental demonstration of achievement of life-test and cryogenic system requirements. This objective necessitates that several magnets be hooked up and operated together. This program will be accompanied by continued production of baseline R&D dipoles in the laboratories. The magnets will be used for the systems tests noted above as well as to measure the variation in their field-quality parameters to verify that this variation in the final ensemble of magnets is consistent with expected distribution.

Development of the collider quadrupole will be continued in FY90, and conceptual and preliminary design for the high-energy booster dipole will be initiated in FY90. The very successful industrially based R&D on superconductors will continue with a focus on production scale-up techniques to meet SSC project needs. The progress achieved so far in increasing the current-carrying capability of the superconducting cable is illustrated in the accompanying figure.

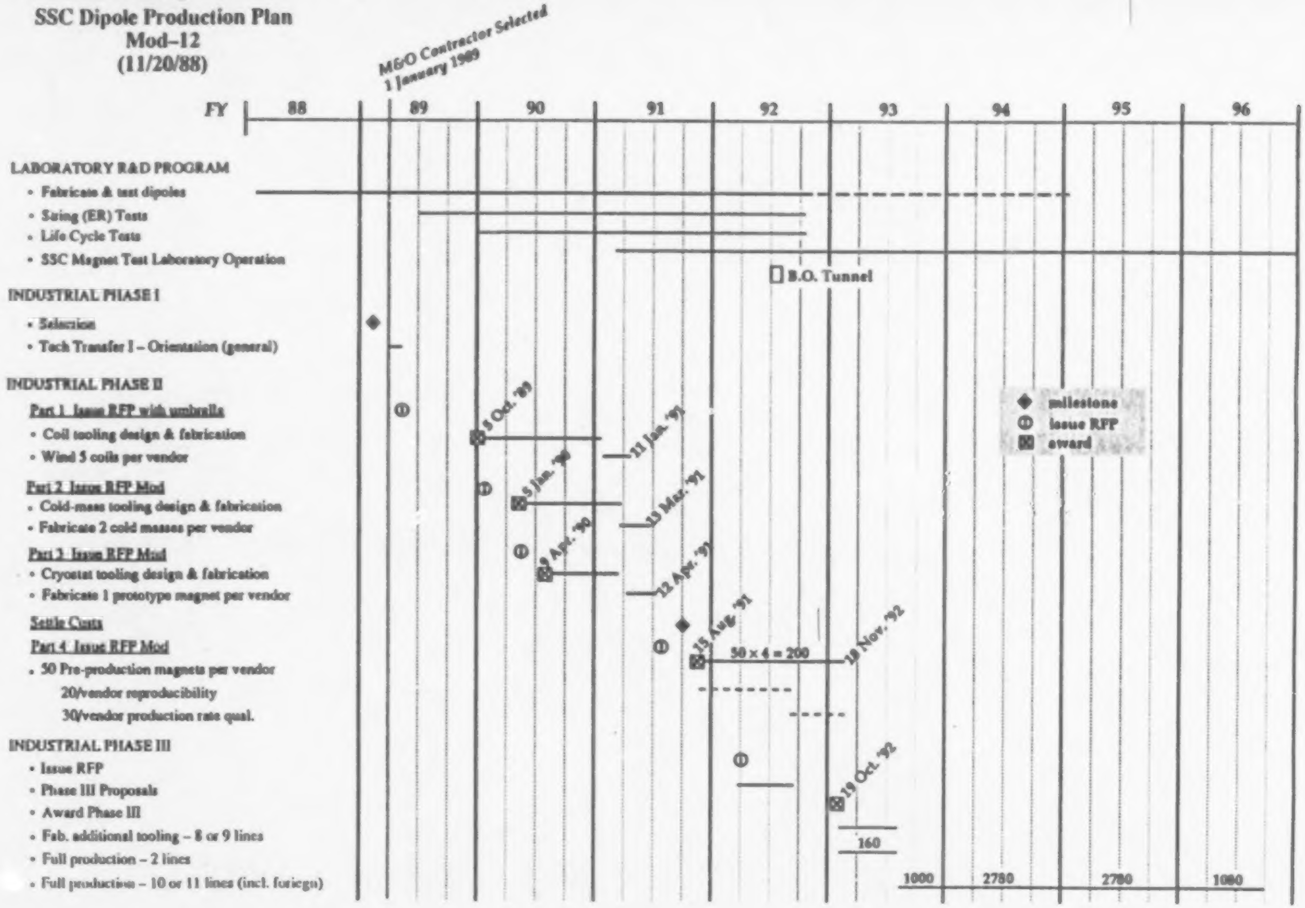
Although the mechanism for the industrial production of coils will be set in motion in FY89, it is expected that the first orders for industrially produced coils will be placed at the beginning of FY90. Current plans call for contracts with four or five vendors to adapt the R&D coil design for production and coil production tooling. These initial contracts for coil production will be followed by contracts for the production of prototype cold masses and cryostats starting toward the end of FY90.

CDG Proposed

SSC Dipole Production Plan

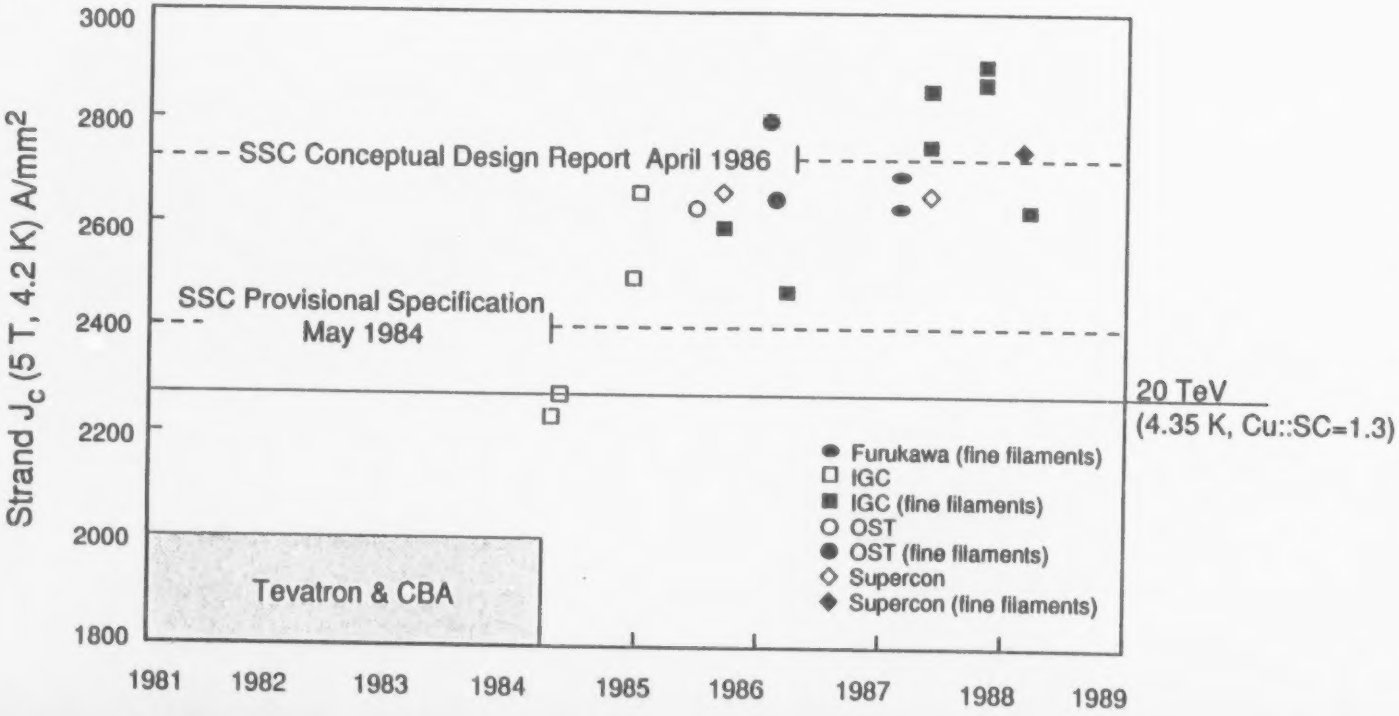
Mod–12

(11/20/88)



Time Development of SSC Superconductor

December 1988



Accelerator Systems. Within this area fall the R&D and design of accelerator components and systems, their integration, and studies and R&D about various phenomena that might affect machine performance. Initially, the design work was largely limited to conceptual designs. Most of the effort was spent in understanding the behavior of accelerator systems and in building tools, such as computer simulation, that will be necessary for the detailed design of the machines. Because the SSC R&D during this phase was concentrated on the superconducting magnets, the Accelerator Systems Division devoted most of its work to systems that interface directly with the magnets. These included, in addition to the magnet systems themselves, cryogenics, vacuum considerations, quench protection, radiation calculations, correction elements, and controls.

The major magnet-systems activities in the past year have been in three areas—dynamic and static thermal calculations, specifying and designing the Magnet Test Laboratory, and integration of the magnets with other systems.

Thermal Calculations: Software has been written to simulate the thermal behavior of the magnet system under cooldown and warmup, and a lumped-element thermal model of the magnet, written under contract by General Dynamics, has been commissioned at the CDG. These studies are particularly important in guiding the choice of design details and materials for the magnet R&D. Insulation and cryogen flow schemes can be studied and evaluated, and the results integrated into the magnet design. The integration effort is significant because the magnet designs are progressing rapidly on many fronts.

The Magnet Test Laboratory (MTL) effort is now far along, and we are reviewing the Request for Proposals before issuance. This facility is intended to be built on or near the SSC site, and to be ready to test 80 prototype magnets from industry during 1991. Eventually, it will be capable of handling 10 to 15 percent of the peak magnet production rate for warm and cold testing.

Cryogenics System: The activities of the past year in cryogenics involved commissioning dynamic simulation software for studying the behavior of the collider ring cryogenic system and establishing useful models of the collider refrigerators in process calculations. The dynamic software was written under contract by Air Products and Chemicals, Inc., and is run on the Lawrence Berkeley Laboratory VAX cluster by CDG personnel. It has been modified by the CDG Accelerator Systems Division to operate interactively on the SUN Microsystems workstations. This software has been found to be extremely useful for investigations of appropriate control strategies to be used in the cryogenic system, and has already shown that some changes must be made. Since it now operates interactively, it will be the base for testing the cryogenic controls system software.

Vacuum Systems: The effort in vacuum systems over the last few years has been to understand the influence of synchrotron radiation on the behavior of the vacuum in the beam tube. In previous years we had demonstrated by experiment at the National Synchrotron Radiation Laboratory at Brookhaven that the pressure rise due to photodesorption will not significantly decrease the luminosity lifetime of the collider. In the past year we have performed a series of experiments at the Synchrotron Radiation Laboratory in Stoughton, Wisconsin to measure independently the photodesorption parameters. These measurements have been published and confirm our previous conclusions. A collaboration with KEK in Japan is continuing to study the effects of magnetic fields on the desorption rates. Preliminary conclusions indicate that the presence of magnetic field is in our favor. An improved experiment will be run in 1989.

Quench Protection: The high-current bypass diodes that would be used for passive quench protection must be operated at liquid helium temperatures in a radiation environment. A CDG–Texas Accelerator Center–Texas A&M University collaboration has been studying the effects of neutron radiation on various diodes. In the past year, it has been shown that at least one diode, the DS6000 of Asea Brown Boveri, Inc. (ABB), is adequate for the SSC, having an effective lifetime greater than 100 years in the presence of neutron fluxes that will be present in the SSC tunnel. We are currently negotiating with ABB to perform other tests that will inform us about the basic reliability of the diode.

Radiation Calculations: We are continuing calculations to understand the radiation intensities, their types, and their spectra in the SSC tunnel and experimental areas. These calculations are important for the accelerator systems and serve to inform us about materials to be used to ensure that electronics can survive in the tunnel. There are numerous collaborations with other institutions, including Fermilab, Oak Ridge National Laboratory, and Livermore National Laboratory. All this work is coordinated and published through the CDG.

Correction Elements: The correction elements are used to correct the systematic and random multipole errors in the dipole and quadrupole magnets. The major effort here has been in two areas—a study of the effectiveness of various correction element schemes and the start of R&D and prototyping work on correction elements. The Correction Element Working Group (CEWG) was formed to study the various possible schemes of correction elements in the collider rings. The first report has been issued, discussing the response of each scheme to expected systematic and random magnet errors and random orbit errors, using both simulation code (TEAPOT) and analytical methods. There appear to be a number of useful and practical correction schemes, but some possibilities are clearly not as good as others. This work will be continuing in more detail in 1989.

Control Systems: Our first efforts to understand the structure of the controls software was begun in 1988. The work concentrated on understanding what sort of database software and general architecture was needed and available, and how one could guarantee compatibility among different types of hardware. A considerable amount of progress was made, particularly associated with constructing databases and communications for data from the magnet tests at Fermilab and Brookhaven National Laboratory.

Accelerator Physics. The accelerator physicists working on the design of the SSC complex have several important tasks: the optimization of the accelerator design itself, including the linac, the injector complex, the main SSC accelerator, and the experimental interaction regions; the detailed specification of performance requirements and tolerances for machine technical components; the development of algorithms and techniques to monitor and control the performance of the accelerator complex so as to maintain the maximum collision rates; and the study of accelerator physics to develop new methods of accelerator design and operation.

The design of the accelerator complex includes the design of the several injectors required for the SSC. The entire injector complex, similar to previous large accelerators, consists of an ion source and a linear accelerator chain, followed by three cascading booster synchrotrons. The proton beam originates in the ion source, is transported between different injector complex components, and is finally injected into the collider ring. Designs have been prepared for the injectors.

The proton beams in the SSC must be very carefully guided around the machine's 53-mile circumference, and they must be tightly focused to remain within the vacuum-pipe aperture. In addition, the machine design must include special experimental regions, as well as long magnet-free regions in which to perform necessary machine functions, such as beam injection, acceleration, and extraction. An arrangement of magnets around the machine circumference that incorporates all of the desired functions has been designed. This design is the result of an extensive optimization of machine performance, ease of operation, reliability, and cost.

An important part of the machine cost optimization is the size of the vacuum-pipe aperture inside the magnet. To minimize the machine cost, the aperture must be as small as possible, consistent with reliable machine operation. Many accelerator calculations and experiments have been performed to determine the minimum acceptable aperture for the SSC.

The design of the experimental sections of the SSC requires highly sophisticated magnet optics to satisfy the different kinds of experimental demands on the beams. Very tightly focused beams are required at the beam-collision points in order to produce the maximum number of proton-proton collisions possible. Arrangements of bending and focusing magnets within the experimental regions have been developed that allow the beams to be compressed at the collision points into cylinders very much smaller than a human hair. By varying the amount of quadrupole focusing in these regions, a wide range of beam conditions at the collision points can be explored, to accommodate a broad range of physics interests. Several different interaction region optical designs have been developed, appropriate for different types of experiments. Additional interaction region designs are under development. The precise configuration and complement of experimental regions in the SSC will be determined in response to requests for experiments from the physics community.

An important difference between the SSC and all previously built accelerators is its extremely large scale, necessitated by the goal of achieving collisions between protons accelerated to twenty trillion electron volts. Because of the size and number of different accelerators in the complex, and the very large number of elements required for the SSC itself, the system tolerances must be tightly set in order to insure the ease and reliability of operation.

To achieve such operation, one must develop a scheme to correct the systematic and random multipole errors in the dipole and quadrupole magnets. Accelerator physicists, together with the members of the Accelerator Systems Division, continue to develop different schemes with which to correct these magnetic errors. Highly efficient correction-element designs are under study.

Conventional Systems. The primary objective of recent work in the Conventional Facilities Division has been to understand the characteristics—particularly the topology and geology—of the Dallas site. In preparation for environmental and land acquisition efforts, the precise location of the magnet quadrupoles was calculated. The layout was then translated and rotated to adapt the footprint to the Texas site. Finally, the locations of the magnets and other components have been determined in the Texas state planar coordinates.

Computerized drawings that capture the site-specific information information on the existing and proposed man-made features at the site and in the near vicinity—electrical utility distribution lines, water supply sources, gas lines, roads, etc.—have been prepared under our direction by the engineering firm, RTK.

The Texas proposal describes a site with attractive geological properties: the ring tunnel will be largely in chalk and marl. Several profile studies of the tunnel in the geological strata have been prepared. Conceptual designs of several structures and enclosures have been undertaken. One example of an underground experimental chamber has been prepared in sufficient detail to allow the estimation of the duration of time for construction. Other studies in this area will continue as the designs of technical components are made final.

The primary technical goal of the conventional systems group for FY 1989 is to prepare a site-specific "footprint" for the SSC. The underground complexes are particularly important, since their placement is the principal factor in determining the final configuration of the SSC.

All the information available about the chosen site will be assembled and a program for collecting supplementary underground geotechnical data and topographic data required to support the conceptual design will be developed. Following the determination of the technical issues, the engineering firm will be expected to undertake a cost estimate for the conventional facilities and to generate a tentative schedule for construction.

A preliminary design for the Magnet Test Laboratory (MTL) to support the testing of superconducting magnets at or near the site by the end of 1990 will be undertaken this fiscal year. Additional work will focus on technical systems and their interfaces with conventional systems, such as the below-ground transport system.

In FY 1990, detailed plans, drawings, specifications, and bid documents for the first underground sectors need to be prepared using the conceptual design and geotechnical data. Current objectives call for beginning work on the following items:

- Near Cluster tunnel, including special sections, such as those needed for injection, rf acceleration and ejection systems.
- Heavy works buildings for component assembly and/or testing (at or near the site).
- Certain advance procurements that will be needed for the increased construction effort in FY 1991.

Schemes for installation of the technical systems, particularly the superconducting magnets, will continue to be refined in FY 1990. Detailed studies need to be made of the sequence of activities that must begin following beneficial occupancy of the enclosures and buildings at the site. A prototype magnet transporter should be acquired and tested.

Administrative systems must be developed and implemented to ensure that the machine configuration is controlled, quality is maintained, and a safe environment is provided for the work force.

The Detectors. To answer our questions about the structure of matter, the SSC must have a high collision rate. About 100 million proton-proton collisions will occur every second at the SSC, some of which will produce hundreds of particles. Identifying these evanescent products requires measuring their energies and directions.

To achieve this goal, many-layered will detectors surround the interaction region. Typically, closest to the collision point, a detector with good spatial resolution will track the many closely spaced particles emerging from the collision. Another layer will measure the curved paths that electrically charged particles follow in a magnetic field. The energy and direction of both charged and neutral particles or groups of closely spaced particles will be measured by the outer layers. In every layer, high spatial resolution will be important to distinguish different products.

Storing permanently the data from every collision would, of course, be impossible. Sophisticated electronics and computer systems will filter the data and discard phenomena that are already well understood, just as in daily life we pay closer attention to unusual occurrences than to the mundane. Only a handful of interesting collisions per second will be recorded permanently.

The SSC's high event rate, the large number of tracks per event, and the need to resolve the tracks of different particles with similar energy and direction present a challenge to detector technology. To meet this challenge, new detection and information-processing techniques must be developed or older methods improved.

Who Is Involved in SSC Detector R&D?

Universities

- Boston University
- California Institute of Technology
- Carnegie Mellon University
- University of Florida
- Florida State University
- Harvard University
- University of Hawaii
- University of Illinois
- University of Indiana
- University of Michigan
- University of Mississippi
- Northeastern University
- University of Notre Dame
- University of Oklahoma
- University of Oregon
- University of Pennsylvania
- University of Pittsburgh
- Princeton University
- Rockefeller University
- Stanford University
- State University of New York at Stony Brook
- University of Tennessee
- Texas A&M University
- UC Berkeley
- UC Davis
- UC San Diego
- UC Santa Cruz
- UCLA
- Yale University

Labs

- Argonne National Laboratory
- Brookhaven National Laboratory
- Fermi National Accelerator Laboratory
- Lawrence Berkeley Laboratory
- Oak Ridge National Laboratory
- Sandia National Laboratory
- Stanford Linear Accelerator Center

Detectors for the SSC must be very large to trap the very high energy particles that the SSC will create: an SSC detector might weigh as much as 50,000 tons and be as tall as a five-story building. Despite this enormous physical scale, research and development in the area of detector technology is carried out by small groups of physicists and engineers at universities and laboratories. Productive collaboration with industries involved in silicon detectors, advanced electronics and computing, scintillating materials, and many other areas is vital.

This year a broad and vigorous program of detector research and development is under way. This program involves twenty-nine universities and seven DOE laboratories, as shown in the accompanying table. In addition, substantial industrial cooperation and collaboration with university and laboratory researchers is taking place in the development of high-speed, two-dimensional silicon detectors, radiation-hardened VLSI electronic circuitry, and new scintillating materials, as well as sophisticated mechanical engineering studies of large detector subsystems and other areas. This generic research and development effort will improve the technological base for design of specific SSC experiments, which is anticipated to begin in FY90.

In FY90 a substantial increase in detector research and development effort and in experiment design activities will be required. Generic research and development on detector technology for the SSC must continue and increase in FY90. This work will continue to be done by small groups in universities and laboratories, working with industry where appropriate. In addition, significant new efforts must begin on the conceptual design of major subsystems of possible detectors, and preliminary work must be initiated on integrating these subsystems into an overall detector design. Fiscal Year 1990 is also the appropriate time to begin to form the large collaborations of physicists, engineers, and technicians that will design and construct the first SSC experiments.

Conclusion

To conclude, I would like to emphasize once more that SSC is a scientific instrument that has been endorsed as essential by the world-wide high-energy physics community. The work on research, development and design that has been performed during the last several years has defined the parameters of the machine, verified its technical feasibility and provided a bottoms-up cost estimate. In short, it gave us evidence that we are ready to go.

The selection of the SSC site and the M&O contractor gives tangible evidence of reality to this project. Together with the dedicated scientists and engineers who have devoted many years of work to this project, we are anxious to begin on this great intellectual adventure, that we believe will be an exciting and promising one. We seek your authorization of this project and look forward to that great moment eight or nine years from now when, with the help of this magnificent new instrument, we can begin to take new steps toward unlocking the secrets of our universe.

Mr. Chairman, this concludes my prepared testimony. I shall be happy to try to answer any questions that you may have.

Senator **Ford**. Thank you, Dr. Schwitters.
Dr. Anderson.

STATEMENT OF DR. PHILIP W. ANDERSON, JOSEPH HENRY PROFESSOR OF PHYSICS, DEPARTMENT OF PHYSICS, PRINCETON UNIVERSITY

Dr. **Anderson**. I would like to condense, if anything, the testimony that I have submitted to Dr. Cooper of your staff, and I thank you very much for the opportunity.

Senator **Ford**. We will put your total statement in the record, Dr. Anderson, and I appreciate your capsuling.

Dr. **Anderson**. Thank you very much for the opportunity to come here and talk.

I am not sure that I thought thank you that much for the opportunity to come in here and talk because it takes a considerable amount of courage for a scientist who has worked on theories. This instrument may or may not test, and for any scientist to come and oppose funding.

Senator **Ford**. Dr. Anderson, this committee admires courage, and so you do not worry a bit about it.

Dr. **Anderson**. And, of course, there is also the problem of appearing to be philistine and anti-intellectual and so on. And so I thought I should say a little bit about who and what I represent and that I actually am a member of what may be a silent majority, even among physicists of people who have real doubts about the SSC project.

And so I thought I would give you as Exhibit A these two books. One of them is the program of the meeting that is the dominant meeting of the American Physical Society of the division to which I belong, the Division of Condensed Matter Physics. It is also attended by a number of other entirely small science subjects like chemical physics, biophysics, and so on.

This is the program of the part of physics that is so much supported by the Department of Energy, the program of the spring meeting at which nuclear physics and which is dominated by nuclear physics and particle physics, but it has of course a considerable admixture of small science in it as well.

One may argue back and forth about the ratio, about whether the papers come in this meeting or that meeting and so on, but I think it is a very good estimate of the size within physics and of the activity within physics to show you this book and that book and to recognize that we really are a considerable majority of all of the physicists and that we are doing a great deal of basic research, much of which is very wisely funded by the Department of Energy.

Incidentally as an exercise in the train coming down here I read the descriptions of support of the different abstracts in this book and in this book, and it was a fascinating exercise.

There are very many Department of Energy acknowledgments in both books, but in talking about cost sharing and the diversity of people who help out with particle physics, I do not think the particle physicists have any idea of how much cost sharing and how much general networking there is between different communities, different industries.

Much of this research, for instance, is carried out by private industry, and yet it is basic research. Much of it is carried out overseas. There are many collaboration and collaborative—much collaborative work between different countries. There is even collaborative work that is funded by two or more countries and so on.

So that part of the reason for the size of this book relative to this book is that there are really a lot more people interested in this in the work that is done in this book.

Now, I am not here to oppose the SSC, but to favor allocation of research funds according to some kind of rational priorities. There are historical reasons why the particle physics community is supported very much more by very much more money per scientist than the rest of the physics community. There are reasons of prestige, which I do not think are very meaningful.

The question is, is there a real intellectual reason and are there real economic reasons, and that is the question that I want to address today.

There is, in fact—I see the basic scientific enterprise, much of the basic scientific enterprise in this country being dismantled primarily because of skewed priorities within the Reagan administration. I see that there is an imbalance of spending between different fields of fundamental science which is increasing, not decreasing, and I see this happening in many fields of science, not only my own. I am not here only to represent condensed matter physics. I see things going wrong in fields as diverse as economics, computer science, hydrodynamics and so on.

In our field cuts up to 35 percent in many grants have been made this year, and after many years of static and/or reduced funding, it is still true that there are a great many faculty jobs going begging because the young people in my field are unwilling to come to universities and face the funding problem, the very severe funding problems that have lasted for so many years.

There are many graduate students coming to universities like my own who are going to have a great deal of trouble finding advisors in fields of practical economic importance to this country, again because of the funding difficulties that make the faculty—make us—that leave these many faculty jobs open.

I see on the bulletin board outside my office a virtual paper storm of job opportunities that are not being filled by young faculty. All of this seems to happen almost inversely to the success or failure of the scientific enterprise which has been involved.

It has been an enormously fruitful period for my field, condensed matter physics, culminated in the discovery of high transition temperature superconductivity which has been referred to several times here. But it is not only that that we have done. It was from this field, for instance, that the fashionable field of neural network theory began and originated many of the new ideas there.

There are many other fields of science we have essentially spun off, so many fields of science that the field has grown much faster than the funding opportunities, and this has not been responded to in the scientific funding process.

It seems that when research produces applicable results, the result is to dry up funding for the relevant fundamental research. It seems that only if applications are not really there, as is true—

or recede indefinitely, as in plasma physics or are not there at all, as in particle physics or space science, that the fundamental science goes on being supported at an equal level of funding.

There are scientists in the fields of condensed matter physics. There are computer scientists who want to make fundamental advances in our understanding of computers as opposed to doing spectacular demonstration projects that the military people like, biophysicists who want to find new understanding of life processes which are not coupled to specific diseases. These are caught between the Scylla of the glamorous big science projects like SSC, the genome, and the space station, and the Charybdis of programmed research, where you have deliverables, where you are asked to do very specific pieces of research aimed at some very short-term goal or at some view of competitiveness which does not represent a clear understanding of how competitiveness really works.

Now, the basic science of today except for some of these basically not applicable fields is the technology of tomorrow. Experience in this country has shown that basic science training is among the best preparations for managing high technology enterprises in a technologically changing world. Thus, you want to—one of the best things one can do is to support fields of science where the ratio of cost to students is low.

The ratio of graduate students produced to cost in high energy physics is three to six times the corresponding ratio in the kind of physics that is represented by this book.

Now, thanks to some negative things that have happened at NSF and some positive things that happened in your planning research within the Department of Energy, the Department of Energy provides an increasing share of the small science basic physics which is funded in this country. This happens to be basic energy sciences, primarily, but also to several of the national laboratories in other ways.

The money not spent on SSC, if there is a definite tradeoff between research funds spent in this way and research funds spent within the Energy Department elsewhere could easily be channeled to better uses.

There are many very good projects that I could name and will not spend the time to detail that the basic energy sciences and the national laboratories fund.

Let me now close, as I did last year, with a remark or two on the question of scientific strategy. Several of the witnesses last year stressed some kind of special virtue for the technical and intellectual athleticism of high energy physics in producing spin-offs and in improving the economic health of our country.

Two of the witnesses before you said that one third of—they quoted Dr. Triblepiece's statement of last year that one third of the gross national product of this country comes from high energy physics. I do not think anyone can possibly justify that statement.

Certainly one third to one half or more of the gross national product of this country comes from basic research, and it does not come from basic research in high energy physics. It comes from basic research in such things as genetics, which is very simple basic research that led to hybridized corn, as just the simplest kind of example. It came from basic research in the quantum physics of solids. That is where the enormous part of GNP that comes from computers and semiconductor physics comes from. It does not come from high energy physics.

At the time that that research was done high energy physics had already split off from the basic physics that was being done by the solid-state physicists.

I think our Japanese competitors have shown, and I sincerely believe and in my own career I have shown by example, that the race can very often be won by doing what comes naturally, by doing the research on something which does not cost all those billions of dollars looking around and doing relatively cheap research on such mundane subjects as glass or conducting oxides.

Senator Gramm has said that the SSC is the most important scientific project to be built in the last half of this century. I wish I was sure of that. I wish I was sure there was not some bright young man dreaming over a desk in a patent office or some monk tending a garden with some peas in it inventing the science of genetics that is doing something infinitely more important than the SSC could ever be.

I think by most important there is a very high—there is the implication here that most important is most costly. It surely is one of the most costly scientific projects of the last half of this century. I have no confidence whatever that it is the most important.

If you ask yourself what will people be referring to a couple of centuries from now as the most important thing that happened scientifically during this period, in any case dollar for dollar we in condensed matter physics have spun off a lot more billions than the particle physicists, and we know it, and we can honestly promise to continue to do so.

[The prepared statement of Dr. Anderson follows:]

OUTLINE TESTIMONY ON THE SSC

P.W. Anderson
Dept. of Physics
Princeton University, Princeton, N.J. 08544
February 24, 1989

I will make my points as briefly as possible, more or less in outline form, and rely for detail on two articles which I have given to Dr. Cooper of your staff: a transcript of a panel discussion under the chairmanship of Barbara Simon on January 17 this year at the AAAS meeting in San Francisco; and an article submitted to the New York Times Magazine by Alexi Assmus. I also have brought along the manuscript I prepared for that panel discussion. Finally I think the material presented by several of your witnesses at last year's hearings is still valid.

How can a theoretical physicist who has actually made contributions to the theories presumably to be tested by the SSC oppose its construction at this time? I am defying two taboos which are very hard to shake: the fear of appearing philistine and anti-intellectual; and the taboo against ever publicly opposing spending on any kind of science, since to a scientist spending on science is presumed to be, by definition, good.

I take this step because I see a vital part of the scientific enterprise in this country being dismantled, primarily because of skewed priorities within the Reagan administration. I see the imbalance of spending between different fields of fundamental science increasing, not decreasing; and I see this happening in many fields of science, not only my own. In my own field of solid state physics, cuts of up to 35% in many grants have been made just this year, after many years of static or reduced funding. It is still true that tens of tenure-track faculty jobs are going begging because of the difficulty of finding support for young faculty, and that graduate students eager to be trained in the fields which are vital to the economic health of the nation are finding it hard to find advisors or be supported in their studies. More students every year want to go into small science fields, and fewer into such fields as particle experiment which are funded as Big Science.

All of this seems to happen almost inversely to the success or failure of the scientific enterprises involved. This has been an enormously fruitful period for condensed matter physics, for instance, culminating in the discovery of high T_c superconductivity, but not only that: in fact, it was from condensed matter physics that the leaders in the fashionable field of neural network theory came, as well as many of those exploiting the new fields of chaos and non-linear dynamics; and these are only a few examples of the many offshoots of our field.

It seems that when research produces applicable results, the effect is to dry up funding for the relevant fundamental research; it seems that only if application continues to recede indefinitely, as in plasma and nuclear physics, or is out of the question, as in particle physics or space science, that fundamental scientists are permitted to continue to spend the public's money. Scientists like myself in the fields of condensed matter physics; or computer scientists who want to make fundamental advances in our understanding of computers as opposed to spectacular demonstration projects; or biophysicists who want new understanding of life processes, not coupled to specific diseases; are caught between the Scylla of the glamorous big-science projects like the SSC, the genome, and the space station; and the Charybdis of programmed research with "deliverables" aimed at some misunderstood view of "competitiveness" or at some unrealistically short-term goal.

The basic science of today—except for particle physics and much of space science—is the technology of tomorrow. Experience in this country has shown that basic science training is among the best preparations for managing high technology enterprises in a technologically changing world; I need only mention such names as Wooldridge, Packard, Wang, Baker, Ross, Varian, etc., to bring this point out. Thus a field like particle experiment where the ratio of cost to students is 3–6 times that in the rest of physics (not to mention the ratio of cost to applicable results) is the worst way to spend a limited budget from the point of view of economic health.

Thanks to the shift in emphasis within NSF, and to far-sighted decisions at several levels, the DOE provides an increasing share of the small-science physics funding in this country, through B.E.S. primarily but also at several of its national laboratories such as Los Alamos, Brookhaven, LBL, Livermore and Argonne. (In some kind of deliberate order.) Money not spent on SSC can thus easily be channeled to better uses, such as the desperately needed upgrade of our national neutron scattering facilities, the excellent work in non-linear science and in biophysics at Los Alamos, a broadened program of individual grants in BES, and many other possibilities such as grants to innovative interdisciplinary centers.

Let me close, as I did last year, with a remark or two on the question of scientific strategy. Several of the witnesses last year stressed some kind of special virtue of the technical and intellectual "athleticism" of high energy physics in producing spinoffs. (This seems a similar fallacy to the myth of spinoff from high-technology weaponry.) I think our Japanese competitors have shown, and I sincerely believe, (and, in my own career, have shown by example) that the race is much more often won by "doing what comes naturally", and looking around at mundane subjects like glass or conducting oxides. Dollar for dollar, we in condensed matter physics have spun off a lot more billions than they have, and we can honestly promise to continue to do so.

THE SUPERCONDUCTING SUPERCOLLIDER

ALEXI ASSMUS
January 1989

The news media—both print and television—have recently taken stock of the superconducting supercollider (SSC). Some have recognized it as a worthy scientific venture but are frightened by the cost. Others have limited their discussion to *where* to build it rather than *whether* to build it, but few have mentioned the raging debate taking place within the physics community on *why* it should be built.

Atomic, molecular, optical, nuclear, condensed-matter, plasma, astro-physicists and even some particle physicists are asking why a particle accelerator with a start-up cost of five to ten billion dollars should be built to serve less than ten percent of the physics community— that is a cost in excess of one-and-one-half million dollars per high-energy particle physicist. Start-up costs in other areas of physics are 100,000 to 500,000 dollars per experiment; in other words, at least ten thousand research groups could begin experiments for the cost of one SSC. At present high-energy physicists comprise three percent of basic research scientists, but they receive 6.6 percent of federal money spent on this research. If the SSC were built, this 6.6 percent would nearly double to thirteen percent.

This prospect has led to great rancor in the physics community. Both at an American Physical Society meeting and in a leading editorial in *Science* (July 8, 1988), Alvin Trivelpiece, publisher of *Science* and SSC supporter, admonished condensed-matter physicists to stop criticizing the SSC. "One should not commit effrontery towards another's project" was one in a list of "ten commandments" for scientists to obey as they seek federal funding for their work. Challenging this view, a physicist from Austin, Texas compared the SSC to a five billion dollar spacecraft that only the Weinbergs, Glashows and Hawkings would be able to ride (*Physics Today*, October 1988). Others pointed out that it might be worth it if "we on Earth would never hear from them again."

This debate within the physics community—which the media has generally missed—is complicated by the fact that particle physicists must justify the SSC project to the public. Experiments have become very expensive, partially because the availability of federal money has shaped the way in which experiments are conceived. No longer do universities support experimental physicists. No physicist can go to his department head and say: "I have an interesting question and I know a way to get an answer experimentally. Will you give me money so I can do it?"

Even if an experiment were inexpensive enough for a university to fund, there is no commitment from universities to any amount of funding. (This is spreading to other disciplines as well: in mathematics, for example, trade-journal advertisements soliciting applications for academic positions often insist on a record of funded research; this was unheard of ten years ago.)

Physicists are therefore forced to apply for grants to fund their experiments. Since they often ask for money from the taxpayer or private industry, these grant applications emphasize the bridge between science and technology, point out the implications of the work for national security, and play on the fear that the United States is losing its first rank status in the scientific world. Proponents of the SSC have expended great energy addressing their arguments to the public, angering many in the physics community who find these arguments self-serving and plain wrong.

There are other reasons, let's call them *private* reasons, even though they may serve much larger cultural ends than the public ones ever might. That physicists and other scientists participate in a cultural enterprise often goes unrecognized. Their private desire to organize physical reality in a meaningful way is akin to other forms of creativity, e.g. musical and literary forms of organizing sounds, words and ideas. Unfortunately the issue of whether or not the cost of the machine is commensurate with its intellectual and cultural benefits has been dealt with cavalierly by the particle physicists who suggest that anyone who cannot see that it *is* worth it is an anti-intellectual clunk. Over the past three years opponents of the SSC have argued against both the public and private reasons for building the SSC in the Letters section of *Physics Today*, the American physics trade journal.

Sheldon Glashow and Leon Lederman, two highly respected physicists, both of whom have contributed to the construction of the standard model—the most comprehensive organizing principle in physics—presented the case for the SSC to the physics community in the March 1985 issue of *Physics Today*. Their reasons for going ahead with the machine were "challenge, spin-off, pride and duty."

Technological spin-offs and pride are public reasons. Glashow and Lederman suggest that building the SSC will generate, as a by-product, new ideas and techniques that can be applied both to the construction of tunnels and of super-conducting magnets for public transportation and electrical transmission. Paradoxically, when estimating costs, particle physicists claim that the SSC will not exceed projected expenses because the machine will be built using existing technology. They cannot have it both ways. Either the builders of the SSC must develop new technology with the ever-present danger of cost overruns, or they must use existing technology, which means they will be a *consumer* of applied science rather than a *producer* of new techniques. In fact the SSC uses old technology; the name "superconducting" in SSC refers to helium-cooled superconducting magnets that have been used for over thirty years, *not* to the new high-temperature superconductors that have recently been discovered by condensed matter physicists. One of the most promising areas for the development of new technology is in the investigation of these new high-temperature superconductors. Ironically, it is condensed-matter physicists who took a twelve percent NSF funding cut in 1988 and are anticipating another one in 1989.

National pride is another public reason. What if Europeans and Americans working at CERN, a high-energy accelerator in Geneva, Switzerland that is open to the world community, were to discover in the 1990's in Europe what Glashow and Lederman want to be discovered in the United States? "When we were children, America did most things best. So it should again," they write in their article. This nationalistic reasoning certainly appeals to Congress, but a broader scientific perspective would suggest that expensive accelerators should be cooperative ventures. Whether or not the SSC is a reasonable project for a group of nations to undertake together, high-energy physicists in the United States are arguing that this country should go it alone.

Within the physics community, however, Glashow and Lederman justify the project by what they call its "challenge" and an appeal to the "duty" to explore the simplest systems that nature has to offer. Their "faith in the underlying simplicity of nature" leads them to believe that the new higher-energy accelerator will allow them to make connections, and thus simplifications, between organizing principles they have developed for lower energy phenomena. A scientific reductionist believes that this organizing "from the bottom up" is the most valuable form of understanding. The accelerator, if built, will push particles to record energies before smashing them against each other to release this energy in great bursts. Phenomenologists, theoretical high-energy physicists who use data from accelerators to construct new theories, believe that the more they can say about small bits of matter (particles), the simpler the world will appear to them.

High-energy physicists argue that the most essential knowledge is that which allows one to make sense of the simplest systems, for example the hydrogen atom or, in this case, the interactions among elementary particles. It has turned out in physics that powerful predictive theories have often been based on a few, elegantly-stated organizing principles (often called physical laws) that are arrived at by looking at these simple systems. These organizing principles are often called "fundamental" because as one makes the system more and more complicated, they remain meaningful and serve as a foundation from which one can begin to understand the more complicated problem. Sometimes, however, the system becomes so intricate that these principles are no longer helpful, and one seeks other organizing principles that operate on higher, more complex levels. Condensed matter physicists are searching for these higher-level organizing principles as they try to understand the new superconductors. No one believes that the particle physicist's account of how electrons interact or present nuclear theory must be changed in order to do this, but rather that other higher-level organizing principles, that do not conflict with the more "fundamental" ones but are not included in them either, must be found.

In fact there is no reason why these higher-level organizing principles should not be called fundamental. Explanations that form a basis or foundation for understanding do not necessarily need to be reductionist. This is not just a semantic disagreement. The word "fundamental" is used to shame people into supporting the SSC, the idea being that if you do not support the SSC you are a person who cannot appreciate this most "fundamental" understanding and its ability to lead to explanations in other areas of science. In fact the flow of knowledge goes both ways. Higher-level organizing principles are suggestive to those working on the simplest systems, e.g. the principle of symmetry breaking which first emerged in condensed matter research is now used by high-energy physicists.

Most telling are the areas in which new physics graduate students choose *not* to work. Experimental high-energy physicists—those who will run the SSC—are having trouble attracting graduate students to man the accelerator facilities that exist even now. Phenomenologists despair at the number of graduate students who are turning away from phenomenology towards string theory—a theory that gets its inspiration from mathematical ideas and larger physical concepts rather than from specific accelerator data. Clearly the young practitioners of this science, who have not invested their careers in an accelerator approach to high energy physics and who do not have to worry about keeping federal grant money flowing into their groups, have cast a vote against the SSC and the sort of physics that it will inspire.

Most physicists want to explore a physical reality outside of themselves and organize it in a satisfying way. Painters, historians, novelists, and musicians explore different realities with the same motives. If we are going to support science for cultural reasons, rather than for nationalistic reasons or in the vain hope of technological spin-offs, we must let those working in the subject make the decision on what research to pursue. The only way we can bequeath to future generations material that will interest, inspire and delight them is to work on problems that interest, inspire and delight us today. For this reason the graduate-student vote is a decisive one.

Society recognizes the importance of these personal, and potentially cultural understandings, but this is not why we support physics at the level we do. The federal government is persuaded by other arguments: we should build the SSC in the hope of technological spin-offs, both non-military and military, or we need the SSC to maintain our status as a first-rate nation in the field of high-energy physics. Most importantly, the government is happy to give money to support existing physicists and to encourage new ones so that the country has a manpower base of physicists who can be called on to solve technical problems involving our national security. National security is the only issue we have not discussed; in fact the debate in the physics community has skirted this issue. Most physicists do not like to think of themselves as salaried workers kept on call by the federal government. In any case they do not want to argue among themselves about who makes the better defense consultants—high-energy physicists or the other ninety percent.

Even as a secondary consideration, funding the SSC for technological spin-offs is a waste of money. The SSC is fifty miles of repetition. For the cost of one SSC we could set up ten thousand groups of researchers with very different approaches working in diverse branches of physics. Or why limit it to physics? If one wants to learn how to build tunnels, let's fund a nationwide project to improve public transportation. Funding the SSC for patriotic reasons runs contrary to the spirit of international cooperation—expensive accelerators are golden opportunities for cooperative projects among nations.

Particle physics will not end with the cancellation of the SSC. There are other theoretical and experimental approaches to high energy physics: theorists are looking at very mathematical ways of organizing the phenomena of elementary particle interactions; experimentalists are doing precision measurements and observing natural cosmic rays or rare decays with large, relatively inexpensive detectors; and astrophysicists are exploring the implications of their observations for particle physics. A diversity of approaches to particle physics should be supported and could be at a fraction of the cost of the SSC.

Department of the History of Science
Harvard University
Cambridge, MA 02138

AAAS Panel Funding and the Academic Physical Sciences
January 17, 1989 Chair: Barbara Simons

BS.: Barbara Simons BR: Burton Richter WT: William Thurston RP: Robert Park RK: Richard Karp RR: Robert Rosenzweig PA: Philip Anderson

BS: We've decided to begin with questions of fundamentals and basics. The first question I would like to ask is why should the Federal government be funding science and technology?

WT: I think that's a good question I think we should stop and think about once in a while instead of only trying to think of what the best way is we can get more money. It is important to stop and ask: Why should society support science? One of the common things people say is, "You have to be careful of what argument you make, or else scientists might be funded at no higher level than historians, for instance". I think history should be supported by society also. So, some of the motivations for current science funding are wrong. One problem is the misconception that science is national security. In some fields the vast proportion of funding comes from the Department of Defense or other military sources. I certainly don't think that defense is the main reason that most scientists do science. It's not the main product that we have to offer the country.

Another fashionable topic at the moment is national competitiveness. I also don't think that's the right way to phase the argument of why society should support science. Science is a very long range project. The ultimate benefits from science is not in a time range of five years or ten years, Especially in my field of mathematics, and also in lots of basic sciences, the direct economic products come in a much longer time range. Science is also an international enterprise. I don't think it particularly props up one country above other countries. It helps, but that's not the main purpose; in the long run promotes economic health. It's also in some way important to the cultural health of the country. Another important reason I think society should support science is education - not so much educating scientists. I think it is equally important to be educating the citizens, the ordinary people of the country of what science is about. This is something that's is disastrous right now, I believe, especially, in mathematics and science in general.

RR: I'll take a crack at that. Societies throughout history have very broadly (invested) in intellectual work in an organized way in the West, certainly as far back as the creation of the Medieval universities, and through patronage of various kinds, through wealth patrons, religious institutions, and most recently through governments. All the reasons why that developed are not entirely clear. But it is clear, at least in one respect, why it has not developed. It has not developed because societies throughout history have thought scientists were particularly attractive people, deserving support in their own right. There has always been some kind of instrumental value associated with the support of science. These scientists in their traditional capacities throughout history have frequently been castigated and vilified as subversive, heretical - various other unhappy appellations. What we do in this country is quite different from the older tradition of supporting science in that, since the Second World War, the support of science and the support of universities has been revolutionized. The nature of universities has had a distinctly more instrumental cast than in previous eras. There is no doubt that were it not for the War, the Cold War, the prospects of curing diseases, the economic condition of the nation, a series of things with which you are all familiar, and for the perception and belief that science and technology have the ability to contribute to the alleviation of those problems, the level of support would not be the same.

Science, after all, existed before the war in a quite different form and at a quite different level than it has taken since the war. So I think the task in dealing with the public support of science - which it now depends on more heavily than ever in history - the task is to blend those directions and motives that individual scientists and groups of scientists technologists bring to their work with the needs of society which funds their work. It's always going produce points of tension. It's going to produce unhappiness on the part of scientists from time to time. It's going to produce unhappiness on part of public institutions and political institutions from time to time. It's going to produce circumstances in which individual scientists are going to have crises of conscience and are going to have to decide what kind of work they want to do, and whether they are prepared to face the prospect of not having their kind of work funded because it doesn't happen to be in favor or because to engage in it at the moment violates their conscience. But it always pays to keep in mind, I think, the fact that the extraordinary richness with which the American people have supported science in the past forty or fifty years comes from their belief that in the aggregate, if not in every specific case, the support of science is going to produce benefits for the society. Those benefits will have to be demonstrated at some point in time, if not immediately, then over a some reasonable period of time. To keep that faith alive, I think scientists have to be conscious of the obligation that they take on when they take on public funding for their work.

BS: I take it you don't agree with the remarks made by Stanford President Kennedy in his keynote address. He was saying that he felt that the American people have lost faith in science because scientists have made promises on which they haven't delivered.

RR: I think that's the other side of the coin. In the scramble for funds, in the desire to get more money, and in the recognition of the political value of the instrumental view of science, scientists, university presidents and various other people have from time to time overpromised. Being oblivious to the consequences of what you are doing is one way to sin. Overstating the likely consequences of what you propose to do is another way to sin. And both of them have the same result which is in the end to undermine and diminish the confidence of the people who, after all, are the source of the riches that fund your work - to undermine their confidence in the value of your work.

PA: I wanted in the first place to emphasize again the wonderful combination of words that Bill Thurston used, economic health rather than economic competitiveness. I wish that Congress and the President of the Academy and everybody else who talks about this thing would use that term. In the first place, economic health makes it clearer that what we are trying to do is to keep up. While we don't necessarily want to outperform the Japanese or the Europeans in this highly competitive way, we darn well better keep up with them, or we will be living in a decreasingly productive, decreasingly satisfactory society. Second, in terms of essentially keeping up one must take a broad look at what is happening to the world as a whole over the long run. There ~~is~~ are not one, but five, six or ten possible scenarios, completely leaving out of the account the possibility of a nuclear war, in which the world as a whole can really go to hell in a hand-basket. .There is not just the carbon dioxide problem, but the disappearance of our usable soil, resources, many, many other consequences of overpopulation. I think we've gotten to a point where if we don't keep running harder and harder we are going to really get into trouble. There is something back there trying to catch up with us, particularly the population problem. If we don't try ourselves to improve our a phrase [illegible]

technological capability we are going to face - ~~somebody in a workshop used the phrase~~ I ~~am going to use~~ - we are going to face a 'universal Calcutta'.

BS: Robert, do you feel there is support in Congress and the government for funding science?

RP: I think definitely so. But it doesn't always translate into as much as we might expect it to in the end. The difficulty is the bureaucracy is organized in such a way that things don't always turn out as good as you think they might. I have sometimes referred to the National Science Foundation as the agency that has no enemies. But that doesn't mean that the National Science Foundation does well on funding every year. Nobody ever speaks ill of the National Science Foundation. But yet, when the final crunch comes, when money from a finite pool is being divided up at the very end, we sometimes don't do very well. Short of reorganizing Congress, I'm not sure there is a way around that. I think that Erich Bloch correctly stated at one point that we keep bad company. We are stuck into an appropriations subcommittee that includes veterans, housing, and things that have very little to do with us, but which are also very important to the nation. So, we don't always do well in that trade-off. But I think that the mood in Congress is pro-science. It always has been.

BS: Do you have any ideas of how we might go about trying to improve the situation?

BR: I don't think the situation is really terribly bad. Look at what's happened to funding of physics. Physics funding has gone up consistently over the last eight years. The National Science Foundation is doing quite well. And if I can correct an impression here, the National Science Foundation budget is mixed up with DOE General Funds budget and NASA's budget in something called Function 250, and not with veteran's affairs and other things...

RP: That's early on in appropriations. In the appropriations subcommittee it falls in with veterans and housing.

BR: And ours falls in rivers and harbors, but that's probably a better place to be. So I think that what we have to do is to keep on with our efforts to convince society that there is something good in science. One of the things that surprised me about the earlier comments is that they were all so practical from all the members of this panel, who are basic scientists, about benefits to the economy and so forth. I think one of the most important things that we have to keep emphasizing is the intellectual benefits of what we do for the human's view of the human and the universe and our place in it. I think that we have a lot to do in explaining that to people. As long as most newspapers have an astrology columns and few of them have physics columns, there is a lot of explaining to do. I think we would all like much more support for our individual sciences. And the only way we are going to get that support is to try and make a case with the funding agencies, with the administration, with Congress, and most importantly, with the public. I think that the public's attitudes have a lot to do with the attitudes of the administration and Congress.

PA: Should we get into this particular argument later on? There was a remark that appeared in the AAAS little magazine, called the Observer. It said "Reagan Years, Good Years For Science." And Burt has repeated that bromide. The Reagan years may have been good years for Burt's science. The Reagan years were miserable years for my science. And in particular the agency that you mentioned as the superb agency is the worst agency for my science. He says the budget for physics has increased. Physics, in fact, is done not in one directorate of the National Science Foundation, but in several directorates in the National Science Foundation. It turned out that the director of the National Science Foundation didn't even know he had any physicists in the division of materials research. Because, when he was told there had been cuts, he denied that firmly, because he didn't know that the part of the NSF that he had cut contained the majority of physicists...more physicists than the actual physics budget contains. The physics budget has recently been satisfactory because it is essentially protected by ~~the~~ its own bureaucratic() community ~~() of~~ in the National Science Foundation. At the same time, the explicit physics budget has been spent on contracting fields. There has been less need for funding of nuclear physics. There is less possibility for funding of experimental high-energy physics because, as machines get bigger, the number of people is getting smaller. On the other hand, the field of physics which I represent, which measured by the size of its meetings or its publications ~~mailing~~ is twice the size of those parts of physics which are funded by the physics budget, has had steadily decreasing funding. Not just per man, but overall within the National Science Foundation. But it has certainly not been true that within physics funding has even been increasing, much less that it has been a healthy situation.

BR: Maybe that's why Barbara put us at opposite ends of the table. (laughter). I'd like to comment on that. I heard a lot about the National Science Foundation and the terrible things that's happened to its budget. But if you look at the global budget for the National Science Foundation, it's up, and it's up very sharply. It seems to me that the quarrel people have is with what the National Science Foundation is doing with money. And I don't think they are doing the right things with the money either. The National Science Foundation, with the full concurrence of the National Science Board, has gone into a massive funding of centers, with a great shift in emphasis to engineering. Whether those are good or bad, I suppose, depends on whether you're getting some of that money or not. But if you ask the global question, which I think is all a Congressman can answer, of how is the National Science Foundation doing, I think its fair to say that "It's doing very well, thank you." But you don't like what they are doing with what they get.

PA: I think we can both agree with that. That's common ground. And I think we can argue, or perhaps you would agree with me in arguing, that a great deal of the fault has come from the National Science Board, which has not lived up to its responsibilities. It has not worked as an advisory board to the director of the National Science Foundation. And it has not served as perhaps the only or one of the very few bodies within the government that could give advice on the overall funding picture.

WT: This remark that the overall budget of the NSF is the only thing that Congress can look at is kind of interesting in light of the fact that the director of the NSF, Erich Bloch, and many other people say that the only reason the NSF has been able to increase its budget is because of these very concentrated initiatives, such as supercomputers and the center program. These highly visible, concentrated initiatives are necessary to sell the NSF to Congress.

RP: I have a fundamental problem here with what's going on. This whole question of whether science has done well in recent years - I don't know exactly what that means. Does that mean that we have done as well as the cost of living? Does it mean that we've grown with the Gross National Product. Even if we had, that's not adequate. I think the obvious fact is that we are living in an increasingly technological society and the scientific base has to keep pace with that increasing technology. The question is, is the investment in the science base keeping up with the increasing technological sophistication of society? That's the only real question. The question of whether we get ten percent this year or five percent the next...that's just absolutely irrelevant. The question is, are we keeping up with the increasing technological sophistication of the society we live in? I think we are not.

RK: I think that although there is a lot of Congressional and public support for science, it's primarily based on what science offers concretely, and it is fairly short term. I'm not so convinced in the existence of a general belief or faith in the long-term prospects of basic science. I think that this instrumentalism is characteristic of the attitude of Congress towards science. They are willing to support the development of things, but are less impressed with the possibility of the development of laws and general insights. I think the exceptions occur in certain fields like elementary particle physics which have been able to bring forth what I call "grand challenges" which somehow excite the imagination of the public. But in other fields which also have an equal claim to the importance of their fundamental work but which have not been able to produce these "grand challenges", I think the instrumental view, the production of products, has taken over as a major rational for support.

RR: I think it's quite true, as I've suggested earlier, that merely extolling the abstract virtues of science would not be sufficient for maintaining the level of science funding that we have seen in this country. But, having said that, it also needs to be said that our politicians don't really understand science any better than I do. (I'm the only non-scientist in this group, unless you want to count political science as a science, which most of you, I think, would not). The politicians have been remarkable constant in their willingness to support fundamental science at a very high level, at a higher level, certainly, than any other Western nation - than any other nation in the world - without really understanding what's going to come of it, but believing that if they do it long enough, that at some point something valuable will come of it. They've been satisfied with that explanation. And so far as I know, they still are satisfied. But that by itself might support the National Science Foundation. It might even support Burt's work. But it wouldn't account for the growth of the National Institutes of Health; it wouldn't account for NASA; it wouldn't account for lots of other things that are now part of the science budget.

I'd like to make a comment on Bob Park's observation that we are not prepared in this country to live in a world of increasing scientific and technological sophistication. I think that's true. I think we would probably all agree to that. But it's not obvious that what follows from that is that we need more money for still more sophisticated science and technology. It may be that the most useful way to respond, the most important way to respond to the present circumstance, is to put more money into elementary and secondary education, to educate a population that is capable of dealing with science and technology in the workplace and in their common lives as citizens. And if one had to choose - if it truly were a zero-sum game - if one had to choose between putting the next dollar into the National Science Foundation budget, putting it into the Department of Education budget, or putting it into the education directorate of the National Science Foundation rather than the physics directorate of the National Science Foundation, the most rational choice for the benefit of the nation and the long-run benefit of science might well be to put it into education. I just put that out as a hypothesis.

BS: Does anybody want to comment on that? Do you all agree? (All seem to assent). Since the subject of education has been brought up, I would like to get on to talking about the universities. Dick, would you like to comment on the problems that you see at universities?

RK: Well I can speak only for the field of computer science, which is a little different from the more established sciences such as physics or chemistry. Computer science is not a well understood field. In fact, if people ask me what I do and I don't want to get into a long hassle, I usually tell them I'm a mathematician, because if I mention that I'm a computer scientist, they'll start asking me about what PC to buy and all kinds of other things I don't know about (laughter). So there are very few members of the public who understand the basic science component of computer science.

What has happened in computer science over the last decade is that there has been a tremendous upsurge in the amount of funding. I think about 60% of the funding for university computer science research comes through DARPA in the Department of Defense. And the bulk of this support has come for the field of experimental computer science. By and large experimental computer science is not seeking fundamental laws. It is creating artifacts. The purpose of experimental computer science largely seems to be to produce demonstrations of technical feasibility. The upsurge in funding has redounded to the benefit of all computer scientists. But with it has come a rise of expectations, which is becoming rather awkward for the faculty, because departments compete with one another. Students expect more support. Students expect workstations on their desks, and so forth. And you get a bifurcation of the computer science department between the rich, well funded projects, and the poorer ones, which are usually concerned with the longer-term, basic aspects of the subject. This produces dilemmas for the students - dilemmas I believe they shouldn't have. It forces students to choose between going in an experimental direction and being well funded with good equipment, and going in the direction that isn't part of the DARPA thrust and having a harder time getting the resources they need. And I think these are problems that the students shouldn't have to face. One of the possible remedies for that is more direct funding for students, rather than filtering the funding through grant investigators.

Another aspect is that faculty members' lives have become much more complicated. I would say that there's been an overall degradation in the quality of their lives, because the quest for funding has made life considerably more frenetic to the point where many students, observing how harassed the faculty are, no longer take them as a model for what they, as students, want to do in the future. So, the upsurge of funding has produced dislocation. On the whole it has been a good thing. But it's not without its problems.

PA: I'd like to confirm what you are saying. I think you are talking about a picture that could be found in some surprisingly different fields. For instance, I think you are describing a situation which is very true of economics. Theoretical economics, which is searching for the basic laws, is a terribly underfunded field. I think that the National Science Foundation puts just a very few million into that. As for practical economics, we have it all around us. An economist who is willing to work for a bank, interested in doing the kinds of things that banks or venture capitalists do, is a remarkably well paid person. A very similar kind of thing happens in the sciences of chaos, turbulence, and hydrodynamics, where the NSF is closing down funding for the most part in that work, as far as I know. Some of the most fundamental work, done by some of the people who are the originators of the modern science of chaos and turbulence, has actually lost funding from the National Science Foundation, where funding for strictly engineering projects - making a strut of some shape and computing the flow around it - is higher than ever. It's remarkable how many fields have this kind of dislocation.

RK: Just as a corollary of what I said, I recently talked to a man who has won all the honors of our field, one of the most respected computer scientists. He is withdrawing from his university position because the hassle of funding has become too much for him.

BS: Would anyone like to respond?

RR: I don't want to respond, because I don't disagree with what's been said. I would like to comment. There is kind of a "Faustian bargain" involved for universities and for scientists in universities with respect to the receipt of federal funds. You lose your virtue quite quickly, and in some respects you do sell your soul if your soul, as you conceive it, depends on or is defined by your ability to pursue independently and without check the work you want to do, at the pace you want to do it. The fact of the matter is that that is no longer possible. The scale and the cost of even the most elementary science is far beyond the capacity of the university, even the wealthiest university, to sustain among its faculty without outside support. So the prewar days of the university as the main patron of science, simply in terms of the time it gave its faculty off from full-time teaching, if you will, that's no longer sufficient to sustain the level of the kind of science the modern world demands. Well, that means you need somebody else to pay for it. And that somebody else in our country has been the federal government. And when you enter into that arrangement, you enter into a demand for accountability, both of an administrative and a fiscal kind, and at least, to some degree, of a substantive kind, that changes the nature of the atmosphere in which you work. You can't do the public's work without paying attention to what the public demands are. That simple formula accounts for the very substantial growth in the university administrations' academic administrative as well as other administrative functions. And when you accompany that with the increasing legal demands for equity that the society has imposed on itself, you have a set of constraints on working scientists, as a consequence of their need for that kind of funding, that has transformed the nature and goal of the conduct of science and the role of the scientist into something that is almost unrecognizable compared to earlier generations. And it has transformed the university, indeed, into a kind of institution that bears in some respects only the most superficial resemblance to the university that existed at the beginning of this century.

RK: I agree that a Faustian bargain is necessary. But it seems to me that there are changes that could be made that would make it less tempting for professors to misrepresent what they are doing in order get money and to seem to fit the missions of agencies. For example, I see no reason why students could not be funded directly on the basis of merit, so that faculty individual investigators would not have the responsibility for supporting so many students through grants. I think that there could be perfectly rational ways for the Federal government to provide infrastructure for the university departments, so that the infrastructure did not have to be pieced together out of the contributions of individual grants that were really not intended for infrastructure. I think another component that requires professors to enter into Faustian bargains is that the universities have backed away from even the most elementary support. I don't think I can get a letter typed at Berkeley without charging it to some grant. So I think if you took the infrastructure, student support, and the very basic operation of the plant, and took care of those separately, and if the agencies were willing to admit that they were not merely meeting their own agency objectives and fostering national defense, but also supporting education and science - and had a little more flexibility in what they would fund - then the temptation to misrepresent might be reduced.

WT: I would like to go back to the question: What is a university for, at least in some idealized sense. What should we aim for as a structure for a university? I think of a university as a private garden for ideas, a nursery where ideas are cultivated. [I also think of it] as a place where students are trained and educated and sent out to other parts of the society, where they use those ideas and contribute in many ways to the health of society. But a university should be a somewhat protected environment.

I think there is too much entrepeneurship that arisen in universities. And [there has been] too much competition between different field [and] different people to get the largest grants. The universities to some extent have abdicated their responsibilities to provide a kind of ideal environment in which different ideas can grow. So one of the problems comes from this increasingly kind of idea of having a planned economy [within] the science establishment.

There seems to be an idea, that [came] after the Apollo mission, that [if] we identify a problem and throw a whole lot of money into it, it will be solved. Scientists, for the most part, have been willing to take as much money as they are able to get in any particular field. I think more money, in some instances, can be destructive. Perhaps we have seen some of these instances. In other words, you have an endeavor which is sort of popular, but not particularly well founded and carefully thought out, and put a whole lot of money in it, which is not really solving the problem. It could draw talent away from many other promising ideas that are not that popular.

Here is another example. Mathematics has traditionally been a field in which it doesn't take a whole lot of money to keep it going. There is a tremendous number of mathematicians who are doing a tremendous number of different research projects. It's really a diverse, productive field, in my view. The total Federal budget for mathematics is about a hundred million dollars. But over the last ten or fifteen years there has been a drastic decline in the number of people going into the field. The number of undergraduate math majors has fallen by a factor of two. The number of graduate students has fallen by a factor of two. The quality of K through 12 education in mathematics has really deteriorated. It's hard to explain this phenomena, except [as competition] from much more lucrative, competing fields [in which] the graduate students are paid much more.

RP: I like the metaphor of a garden. But for my garden to flourish I have to dig manure into the soil. I think part of the problem is that faculty members are scrambling for grants. And that's certainly true. When the amount of time you are spending raising the money begins to exceed the amount of time you are spending doing research with the money, things are beginning to get seriously out of balance. That's what we really have to worry about. The idealization of the university is nice. But, the fact is what we are missing is long-range stability in the research that we do. It's not that the university should not respond to societal needs. Of course they must do so. Society has every reason to expect that. But we need to provide some assurance of long-term support for the things that we undertake. It's the short-term nature of the support we give, I think, that is causing the difficulties we are hearing here today.

RK: I think another aspect of this is the question of whether the kinds of scientific work that get funded would promote the graduate educational mission. When I was a young idealistic student, I was told that the purpose of the Ph.D. dissertation was to make a contribution to knowledge. When I looked at the rather large hierarchically organized projects producing software and hardware, projects which I see in my own field, project with as many as 20 to 25 students in some cases - each student carrying out a relatively modest assignment within the overall structure and eventually getting a Ph.D. for it - I wonder if the notion of the Ph.D. as a contribution to knowledge is really surviving. It seems to me that if ever in a scientific career one should have the independence, the autonomy to pick, to form one's own style, and to pursue the questions that motivate the person as an individual, it should be the at stage of Ph.D. training. What we see instead in many cases is the insertion of people into large projects which prepare them to run off and run other large projects. Sometimes this is necessary. But it cuts in some ways against the traditional purposes of the Ph.D.

RR: If it weren't for the fact that graduate are trained in the course of doing research, as we organize research in this country, there would be no particular reason for doing research in the universities. It could just as well be done in independent laboratories, in industry, and in government labs - maybe at an American Max Planck institute. But [it might] not necessarily [be done] in the universities. It's the training function of the university, the educational functions of the university, that we have used as the justification for locating the conduct of research in those institutions called universities. I hear a fairly fundamental questioning of that premise.

As a matter of fact I heard a paper yesterday at a different session in which the author took a close look at the way the system actually operates in a department of physics. The paper is a preliminary report of a larger study in which a department of physics is compared to a department of history in an unnamed university. Some very troubling questions are raised in the paper about whether education is really taking place in either department. If it's the case that we are doing badly or that the pressures of the time are forcing institutions and academic departments to do badly what it is they are actually supposed to be doing best, mainly combining research and the education of graduate students, then we really ought to be rethink fundamentally the whole system. I wonder if the university is really the best place to locate the conduct of so much research.

BS: Picking up on your comment I would like to ask whether the dominance of mission funding agencies in the funding of science at universities has some effect on the universities?

PA: I did want to make two remarks. First, if you remove research from the universities, you are very rapidly going to make the educational process even worse than it already is. Because the people who are aware of what's going, people who are on the cutting edge of knowledge, will no longer be in the university. That can become a very sad situation. I certainly am a foe of direct mission funding, even though I work in a field that does a great deal of highly practical stuff. In fact, for most of my life I worked primarily for an industrial laboratory, and we did a great deal of practical stuff. What I experienced in that industrial laboratory was a much longer-range, much wiser proportioning of the job of getting a particular mission done, and much more of a sense that there is in the beginning a wide-open area where one doesn't really know exactly what one is up to. One must be very carefully to avoid rigidifying the goals, setting deadlines, and so on. And then there is a point later on in engineering, at the engineering level, where of course you have to stick to those deadlines, goals and so on.

What has happened a great deal with government funding of academia is [that] the government [is] seeing the second kind of operation as the way to manage research. There's one which I've encountered repeatedly within the materials research community, the materials research laboratories concept of a "thrust". We have materials research laboratories in seven or eight universities across the country which are very good. And they do, in fact, fund a great deal of materials research physics that might perhaps be better funded under other auspices. But, over three-quarters, perhaps eighty percent of that funding must come under the head of certain "thrusts". There is a particular object that must be produced. I understand from people who have DARPA funding that this is what DARPA calls a "deliverable". They have a schedule for a deliverable that that program must come up with. For instance, [if you are] looking at oxides and you happen to come across a new superconductor, that would not be allowable under a DARPA contract, and you would have to immediately put it aside and forget about it. It would not be part of your 'deliverables'. And, really a very large fraction of research, most of it should be going on in the universities. It should not be held to this kind of scheduling, to this kind of direct funding.

It's worst in my field of superconductivity because that's a field that's only two years old. And we are already being asked to make thrusts and deliverables and to do extremely naive things with these superconductors, long before we have the faintest idea about how they work and how we should really be using them. And the money for the fundamental research behind them is steadily being cut in favor of money that is going into DARPA's schemes for consortiums, various forms of programmed research. The wisest man in the world doesn't have the faintest idea of how to ~~program~~ micromanage a research program in superconductivity. I know very well what is going on the industrial laboratories involved in this field such as DuPont, IBM, Bell Labs, and Bellcore; they're as far as possible from programming the research and asking for deliverables, etc. They are doing a little bit of looking at actual devices. The great majority of the research is at the broad, unprogrammed level. And it should be.

BR: I think your question was about research in mission-oriented agencies, as well as the overly directed research in non-mission-oriented agencies. I think that depends on the agency. The Department of Energy, from which high-energy physics gets almost all of it money, and from which nuclear physics gets almost of its money, has been rather good about not trying to focus research too tightly or direct it too centrally. I don't know how to handle mathematics in DOE. Maybe they are bad guys like the other agencies are....

WT: For the most part they don't fund mathematics.

BR: They have a little, not much. But in the basic energy sciences also, from what my colleagues report to me, they are pretty reasonable. Some of the other agencies have changed a lot over time. The military used to fund basic research in a rather generous fashion. But I think that it was the furor of the sixties and seventies and partly the academic community that got them out of that. The Mansfield Amendment, I think, was something that was very destructive for the whole basic research enterprise. It forced the military to do just the kind of thing that Phil doesn't like DARPA doing. I agree that it's not healthy for DARPA too. They have to find now either a fake or real military justification for what they are doing, instead of supporting the basic research in the broad sense because technology will come out of it that is in some sense beneficial. I wonder how much we all had to do with that change. It certainly wasn't good for us.

BS: Was the problem with the Mansfield Amendment the intent or the fact that the money didn't get switched to organizations like the National Science Foundation.

BR: The Mansfield Amendment, even though it never passed, changed a lot of thinking. It simply said that everything you do has to have a specific military justification.

BS: If you are receiving funding from the military.

BR: That was in force for only one year. It is not in force now. A modified version is. But, that's an interesting example of the value, if you will, of mission agency research. Mike Mansfield's idea was that if you are doing basic science, put the money in the National Science Foundation; get it out of the Defense Department. But it never went to the National Science Foundation, obviously, and it never would. And no single agency responsible for university based research, say, much less all of basic research, would get the kind of funding that we now get out of the six major agencies that fund research in the universities. You have to recognize that the things that you don't like about the system are, in some respects, a natural fall-out of the things you do like about the system. What you want to do is to improve the things you don't like incrementally, without throwing away the whole system that gives you a lot of the things you do like, which in our case is a heck of a lot of money for the conduct of research.

BS: Is there an element of dishonesty in trying to get funding for basic research, say from the DOD, when Congress wouldn't give that money to the NSF?

RP: I don't see why. It seems to me, as Burton said, that the Dept. of Defense ought to face up, and some people in the Dept. try to face up, to their responsibility to fund basic research and to their responsibility to fund infrastructure that supports basic research. They can't simply be consumers; the way our system operates they have to be investors as well. I think they have fallen short, far short, of their investment responsibilities, and they ought to be pressed to meet them.

RK: I think that there is a built in encouragement to dishonesty. For example, if you look at DARPA in relation to computer science, the biggest source of funds right now is the Strategic Computing Initiative at a hundred million dollars a year. That's 6.2 money, which means that it's advanced technology rather than basic research. And it's linked to certain military missions such as expert systems for battle management and land-based robotic vehicles.

I think that all up and down the line people are telling two stories, one to people above them, another to people below and to themselves. I think the guys in DARPA who really have done a wonderful job for the field defend that program to Congress in much more instrumental and military oriented way, while at the same time trying to use the monies that come out of it to foster the health of university departments and to get some generic research going. I think people in the universities who want a piece of the SCI action are almost overwhelmingly forced to masquerade what their doing as related to these specific missions, such as battle management, when in fact what they want to be doing is generic work in artificial intelligence. And if you really tell it like it is - either at agency level talking to higher-ups in the military, or the military talking to Congress, or to the agencies - if you really tell it like it is, you might just be ought of luck. So there is just an awful lot of duplicity arising as a consequence of the funding process.

WT: Right. I think this goes back to the picture of what a university is for. I agree that we need a certain amount of manure. But I think we find too much of this bullshit. So this is an issue which I'm very concerned with. I don't quite follow the logic of the argument that because we have a large component of military funding in universities, because it's been that way since World War II, that that's the only way it can be. The United States is rather unique in having such a large component of military funding of science. For instance, in mathematics I think about half the Federal funding comes from military sources. I think [the funding results in] far less than half of the scientific output of mathematics. I think most of that comes from the much broader range of research funded by the National Science Foundation.

I think that in some ways there is kind of an apparent contradiction between the ultimate purpose of the military and the ultimate purpose of the university. The ultimate purpose of the military is to somehow enforce the will of the country by power, by superior force. Somehow, the idea of the university as a kind of protected environment, where ideas can compete with each other on a level playing field, doesn't really go along with that. At various times in history there have been insulating barriers between this sort of mission oriented aim of the armed forces and the aims of the university. I think that ONR after World War II took a very enlightened point of view, and tried to do something productive to convert the vast machine that the United States built up to fight the war into something that could work for the long-term. But I think that by now this has degraded. I think that the scientific community should try to do something to wean itself from its dependence on the military.

PA: Without necessarily coming out against duplicity, old-fashioned values these days are beginning to be fashionable again. I should remark that there was a semantic confusion in one of the previous speakers, at least, who said that the funding system causes the duplicity. Let's make it quite clear that it is we who are duplicitous, the funding system encourages the duplicity. How we feel about the duplicity is a matter for our own consciences. But I think it's relevant for us to recognize that we are doing these things.

BR: I'd like to come back to your earlier question about research and the mission agencies. I've always thought that it was very important to have research support distributed broadly through the government, because everything our government does these days has a science and technical component to it, whether you are talking about the Department of Energy, the Department of Education, or the Department of Agriculture, or yes indeed, the the Department of Defense. I believe that the people who run those agencies have to have some intimate contact with what's going on in science and engineering in those areas that are relevant to their mission so they can carry out it out in a sensible way. That is not to say that they should be directing scientific programs in a microscopic fashion toward their ends. But they have to know what's out there that they can call on. So I believe that it's important, however we reorganize science funding - I presume we will get into that eventually - that we maintain a system where those agencies which have a mission have a component of science support in them, if only to keep them current with what's going on in the scientific world.

BS: If you take the example of computer science, the field that I am familiar with, would you say that the funding of academic computer science by the DoD - mainly DARPA - at a level of close to 70% reflects the need of the DOD agency as opposed to the other agencies in the government?

BR: Well it certainly reflects some need of the DOD. But perhaps you could apply for support to the Internal Revenue Service. They need a lot too.

RK: I think the question is how to shake out of the local extremum we are in. It seems that society moves by small increments. And agencies will naturally do everything possibly to stay in business and to extend their power. So, I don't see a reasonable scenario under which the basic research mission would be shifted out of the defense agencies and into some other agency, or in which the concern for economic health will be shifted into some other agency. So I think we are facing the question of whether we should accept the overall structure and work within in, or whether we can really believe that somehow massive structural changes are possible. I don't know the answer to that.

PA: I would like to hear Bob Park's remarks on that....on whether structural changes are possible.

RP: It's certainly very difficult. Enormous inertia is built into the system. And every time you try to make some sort of correction within the system some program shows a large percent increase that year. By the time they get around to the final stages of the appropriations process, and they are looking for additional funds, they start looking where the percentages are the largest. And so we are kind of caught in this incremental funding that makes it very difficult to ever change the picture substantially.

I think we have gotten out of balance. I have no doubt of it. And I think it's not even good for national security purposes to be this far out of balance. The Young Commission report a few years ago made one of the most profound observations in a long time, and it got ignored by almost everyone: the Department of Defense is a net consumer of technologies, not a producer. And, the result is, you've got to have the rest of the system healthy in order to benefit the DOD. They're getting their technologies from IBM, Bell Laboratories, and other places. And the universities' job is to train the people who do these things.

We have the case of the Stealth bomber, which was really stealthy. This came on us without our ever hearing about it. And yet they are talking about a $70 billion program to have gotten this far. [The Stealth] took a virtual army of scientists and technologists, using as an information base built up over a great many years. At some point you've got to start investment in system that produced all that. If we keep diverting our technical resources to these kinds of enormous projects and don't put an equal investment into science base that supports it, I think we're headed for disaster.

BS: Now I would like to open up the floor for questions.

#1: I'm David Lazarus, from the University of Illinois, and also ~~(??somebody coughed) in~~ the American Physical Society. Those of us of a certain age remember when there was zero federal support of science. And yet there was an awful lot of science. In fact the present pattern arose - I can date it precisely - there was one person, Manny Piore, in 1946, who was a civilian with the Navy Department. He persuaded the admirals to put up a few million dollars to establish the Office of Naval Research. It became then the first funding agency. All the funding agencies have arisen probably in the ONR pattern. The idea was to take unsolicited proposals from meritorious people and decid~~ing~~e on the basis of merit. It saved my own neck as a graduate student. We got a thousand dollars at the University of Chicago, where I was then, to buy the timing oscilloscope which I was about to spend a year and a half building out of war surplus products. We should really not forget our history, or we will be stuck like Santilliano(??) had said. I have to agree with Dr. Rosenzweig. We entered innocently a Faustian bargain at that point. And I'm afraid we lost. We started with a budget, I would guess probably at ONR of maybe $5 million a year, and we are now talking about some billions. There was no NASA, no NSF, no other laboratories, no DOE, no AEC. All these things came later. ~~And all the research patterns originally (???)~~. And the universities, of course, loved it.

We started with a $1,000 grant. My thesis advisor, (???) with the oscilloscope.... and it got to be sort of an entitlement. And then we started building buildings, which my own university's profited by enormously. My own university is probably a big abuser of the project thing. We've got a center for supercomputer applications. We have a center for supercomputer development. We have a materials research laboratory. We have a superconductor research center. [We have] all of these things supposedly because of this funded research going on at the University of Illinois. But we were a university.

And there was an awful lot of physics before there was five cents in federal funding. Relativity was developed without the cyclotron, the betatron, all of the nuclear physics. Physical Review in 1946 when I was a graduate student was only a half an inch a month. And yet the interval between major discoveries in physics was maybe one decade. I am now unfortunately editor-in-chief of the American Physical Society. The Physical Review is now two feet a month. The interval between major discoveries in physics does not scale anywhere near in proportion to the amount of money that has gone into it to the number of practitioners. What's happened is that we have a hundred times as much money and a hundred times as many people in physics, but we have not contracted by a hundred the interval between discoveries. As I say, I am not sure we have won this bargain.

And we may be on the wrong side. If my mother were still alive, she would probably say, "The trouble with you people is that you're spoiled rotten."

PA: This is a topic I have thought of several times. I think the statement has been made that there was no federal funding of science before 1940. Since this is a symposium of physical scientists, that may to some extent be true. It wasn't quite as true as Dave makes it out. In fact, the entire system of agricultural experiment stations, not Federal but state in particular, the university, [illegible] at which Dave resides, ~~was~~ were a result of a massive piece of Federal funding during the 1870's for exactly the purpose of fostering science and for improving the most important technology in the country at that time.

Near the university, when I was young, there was another institution call the Geological Survey. It was a federal institution that funded rather massively geological research, which was considered to be very important, and a very important technology. It is a very important technology that to some extent has been taken over by private industry. Industry was funding science at a pretty hefty level at the time we are talking about. And that of course, is another subject, but something that we should talk about. Why is that private industry is dropping out of the science funding picture? Why is that these wonderful laboratories are blinking out one by one, all over the country?

RP: I would like to say one thing about what Dave said, which I agree with, but which might be slightly overstated. There is an aspect to this, to go back to something I mentioned earlier, a 'sophistication inflation'. Each time you push the frontiers of science a little farther, it costs a little more to take the same step. I think that it was fine that people could build accelerators in those days for very few dollars. But those accelerators won't find us any new information today. I think that if you just look at the cost of accelerators...I've noticed that they've gotten bigger. There is a sophistication inflation involved and I think that that's unavoidable.

RK: I agree that there was a lot of science going on before the war. I don't think we can go back to those golden days. We are kind of stuck with the present system.

I would like to talk a little bit about industrial funding. Because I found in my own field, perhaps because our appetites have gotten so big, maybe we are kind of spoiled. Computer science and engineering departments have begun going after large doses of industrial funding in areas like VLSI technology, semiconductors and so forth. And, there is obviously a lot to be gained intellectually by interacting with industry, because there are some really first rate problems in the fabrication of materials, in logic design, and so forth. But what I found to my dismay is that the industrial sponsors are much tougher task master than even the hardest nosed mission oriented agencies. It's kind of a contradiction, because you have a few places like Bell Labs and IBM where very pure science is done. When I left IBM for the University of California, I was really leaving the ivory tower. But if you look overall at the kinds of controls and monitoring that the various industrial consortia that sponsor university work have imposed on the investigators, it goes far beyond what DARPA or the other DOD agencies require.

PA: Dave, I should answer one other thing, which is your point about basic research. I have a slide, which I don't have with me, of major discoveries in condensed matter physics in which I am able to find one per year in both theory and experiment. It's not a hundred times as fast, but it certainly is ten times as fast. If you ask whether Nobel prizes are being given any more for lighthouses ~~(??garbled)~~, no they are not.

#2: Rolf Sinclair from the National Science Foundation, currently on leave from Los Alamos. I think that it's a pity that up among you six people there is not a seventh who is representing one of the agencies. I sort of stopped making notes after a while on the micro and macro confusions between agencies and sort of trivial mistakes that seem to color an awful lot of what was said. I'm not going to try to go back through all of that. I think that it's a pity that they were not discussed in real time.

But I'd like to make one point about the confusion of the roles of the agencies. Congress sets the roles of the various departments and agencies of the government. The agencies do not set their own roles. They are not free to do this or that. Col. Whitakker, who headed DARPA, came to the Physical Society and gave a talk, and said, "Please do not confuse the Department of Defense with the Department of Education. They have very different roles". The various agencies will contract for the research that they feel necessary. And the universities are one source of that contracting. They do it with full knowledge that they are supporting this structure. And there is a tremendous amount of sympathy on the parts of the program officers in the various non-NSF agencies to keep that structure going.

But there is always a conflict in the zero-sum game. You have to justify why you should do this and that to support the industry and the educational process. You have to redefine the roles of those agencies through Congressional action before you can do anything else. The Congress has set up NSF to support research, you might say, for its cultural value, that is, for no particular mission. We are approached by people who want to make end runs on the mission budgets. We want people to make end runs on the NSF budget. People come to us and say, for example, would you support some work in fusion because we cannot get DOE to support it. The answer is no. That's DOE's mandate. Would you support this work in space? No, that's NASA's mandate. NSF has its own mandate and has a certain amount given to it by Congress, and no more. But the other agencies are there for specific reasons, part, and only part, of which is the funding of academic work. I don't think that as much duplicity is coming into the process as perhaps many of you feel, with all due respect. Because, the reason for doing the research in the heart of the scientist does not have to be the reason why the agency supports it. It's perfectly good to do something that's perfectly basic if an agency recognizes its need. If it does not recognize its need, it cannot support it, period. I would rather have it focused more on what the agencies are trying rather than on what in some realistic world they might do.

RR: In the real world, in which I think I live most of the time, what agencies do is a complicated interaction between the people who manage the agencies - the professional civil servants and their political masters - and the people with whom the agency works - its clients, its constituents - which in our case are scientists and technologists who interact with the agency people constantly and who find in that interaction what it is that the agency is going to support in the next year, and the year after that. The agency bureaucrats, their political masters, and their constituents undertake to persuade [Congress] every year through the budget process to what that mission ought to be. So it's not as if missions were fixed in or cast in stone when the authorizing legislation was passed. In fact, with not total latitude, but with substantial flexibility, they are shaped and reshaped over time as a product of those interactions. And that makes a much more complicated and more interesting and much more useful system I think than the more idealized kind, either what you heard here or what you described yourself.

RK: I think a large part of the dilemma arises precisely because some of the agencies attempt to go beyond their charter. For example, there is no doubt that in the seventies DARPA was very consciously attempting to promote the health of certain computer science departments and the growth of computer science. There is no doubt either that whatever its Congressional charter may be, DARPA is presently attempting to gain a foothold in support of the economic health, for instance, or economic competitiveness, by supporting projects in high-definition TV, and so on. And there is always a rationalization given. But I think the model of the agency as simply faithfully carrying out its charter is perhaps idealized. And there is the other element of the agency trying to extend its power. If the agencies were simply fulfilling their charters, then the case would be clearer for increased support for computer science and increased support of civilian agencies. A dilemma arises because de facto they are extending their charter into the pure research and civilian areas, therefore complicating the picture. We are then faced with the dilemma of whether to try to encourage this and get the most out of it, or whether to resist it and hope that some new agency will spring up to support basic science and economic health.

PA: I think, in the first place, we did ask for an administration member of the panel. I don't know how that broke down.

BS: We were thinking up to the very end that we might be getting someone from the administration. First I was trying to get Bill Graham [Pres. Reagan's Science Advisor]. And then they were talking about sending Bob Post, who had been in the [Office of Management and Budget and is now in the Office of Science and Technology Policy]. But OSTP did not have the money to send him.

PA: I want to go back to your statement that the NSF has as its charter the support of basic science. What I've been trying to say, and it certainly happened in my field and in several fields that I could mention, is that the NSF has abandoned that charter and is trying to do what the mission agencies are. And therefore it is being left to the mission agencies to carry out the charter that should have been the NSF's. In my field basic research has been cut back by the NSF in favor of applied research in the field of hydrodynamics and turbulence. Basic research is being cut back by the NSF in favor of applied engineering research. So NSF is not supporting the basic science it should be supporting. Therefore, we are forced to become duplicitous...we aren't forced to become duplicitous, but we tend to become duplicitous in trying to find funding in mission-oriented agencies, because we feel that NSF is not carrying out its mission. The cuts in basic superconductivity research are 35% on theoretical physicists, this year.

#3: William Palmer, the Ohio State University. Hearing Professor Anderson this morning, and contrasting his point of view concerning the role of the NSF with a whole other world over at the Hilton who spoke yesterday about competitiveness, about technology transfer, about interdisciplinary studies, about the rigid, old, ossified university system which is no longer contributing to the public good...hearing that, and hearing the kind of turmoil which must exist at the NSF, I'm so grateful that I've been funded for the last twenty years by the Department of Energy, under its high-energy physics program, which, by contrast, enjoys the general consensus of a smaller community which has agreed upon its goal in life. I do have a question which is: how will these two cultures be combined under the umbrella of the NSF, and should they be combined under the umbrella of the NSF? One culture is Phil Anderson's, and Bell Lab was clearly part of that ivory tower as he described it. And that other culture is the very, very able people in industry doing good science who strongly feel that it is the mission of the National Science Foundation to further connections between industry through its science and technology centers, through engineering research centers, and other kinds of modes of funding.

PA: The one science and technology center which has been set up in superconductivity, I should remark, has virtually no industrial connections. Its industrial connection is via Argonne National Laboratory, which I thought was a DOE laboratory. The major support for that science and technology center, that is its so-called interdisciplinary center, is via the government laboratory. That center does not have close contact with the major industrial players.

RR: There are clearly tensions and uneasiness within NSF and within the community that NSF supports as a result of the change of direction that the Foundation took, maybe five years ago. But it's important to remember that that change of direction was quite consciously taken as an explicit alternative to a very strong movement that was within the Congress at the time to create a new agency for the support of technology. And the decision of most people, certainly one that I concurred with at the time - although my opinion wasn't asked - I thought it was the right decision. It was better for the development for both kinds of technology if they went forward together in the same agency. That may not have been the right decision if we look back. With fifteen years of perspective it may turn out to have been the wrong decision. But, I think it's too early yet to judge.

RK: I think it's worth noting that the center concept so far hasn't worked out very well. NSF has been sponsoring these engineering research centers and science and technology centers.

One of the reasons it hasn't worked out well is that the level of funding is too low, with the consequence that in the case of the science and technology centers there were 342 proposals submitted and only 11 granted. That represents an enormous waste of manpower.

But beyond that, I think that another reason is that once you get something - once you get these big lumps of research, as Phil Anderson has referred to them, these big chunks to do interdisciplinary research - the process gets very political for a couple of reasons. First of all, there's a lot of money involved. So you get university administrators lobbying for their own universities on non-scientific grounds. Secondly, you have mixed criteria, because the criteria for choosing a center are related to scientific excellence, but also to geographic distribution, to the development of universities, to university-industry relations, etc. And when you get all these multiple criteria, many different courses of action can be rationalized and defined, and then politics is free to take over. I think that this is what to a large extent is what has happened in the case of the centers. So maybe in the future this can be fixed up. But the first venture in this direction hasn't worked out terribly well.

#4: Let me first of all introduce myself. My name is Jim Krumhansl. I am a professor of physics at Cornell University, theoretical condensed matter physics, and president of the American Physical Society, and for two and a half years I was assistant director of the National Science Foundation, for the physical sciences, astronomy, ~~(2~~)mathematics and ~~in~~ engineering. I see the confluence of a number of different messages here. The discussion has been very useful from the point of view of trying to look at the fundamentals of how to establish a reasonable ~~basic~~ rational basis for the support of science. But there's a problem. When you begin to look at the data in detail, sometimes from the bottom up, you see quite a different image than you do from the top down. Professor Richter says that the support of physics has been moderately increasing over the years. On the other hand, if you look at the division of the enterprises in the Physical Society, we have divisions for particle physics, condensed matter physics, so on and so forth, you'll find data that absolutely corroborates what Phil Anderson is saying. The fluid dynamicists are badly underfunded. In astronomy, many of the facilities are having to lay off tens of their staff, their support staff, and so on and so forth.

Now in part, this has to do with the difficulty, again, of data analysis. If you look at the overall NSF budget, for example, there are appearances in the aggregate figure of health. On the other hand, because of this contact with physics, in particular, in its needs, fluid dynamics, atomic molecular optical physics, condensed matter physics, nuclear physics, fiber physics, and so on, across the line, where we find this story as far as un-mission oriented science or non-center oriented science is concerned is in that part of the budget which is the plan by particular division. If you analyze the last four years for almost all the division of the physical sciences and fluid dynamics in the engineering division, you will find that in fact funding has gone down steadily. It has not kept up with the cost of living by approximately 3-4% in terms of constant dollars. So, on the one hand we have a story of a rich city, but we have number of homeless cases developing. And what I'm arguing for is that when we are having these discussions we have both the long term view of nutrition involved, but we also have short-term therapy. And when we are doing short-term therapy we better do the right diagnostics.

BR: I would like to make a couple of comments. I also have observed what you are talking about in the NSF budget. In broad support of basic research it is going down. In the total aggregate amount, the NSF budget is going up a very large amount. I have for several years asked myself why. And I have not come up with any good answer. I am not funded by the NSF. I'm not part of the in-group that talks to them. I am not a member of the National Science Board. Something is certainly wrong. Now, I also for a scientist maybe have an odd belief that the physicists should never get as much money as they ask for. The only way to force scientists, mathematicians are different, the only way to force scientists to make choices, and I think choices have to be made, is to put some budgetary pressure on the scientific communities. Push them to start setting some priorities.

We don't do a great deal of old fashioned spectroscopy anymore. That's probably a good thing. We do other things. We do laser physics instead. Somehow the scientists have got to do something to make some changes in what gets supported, because there is never enough money for us. I could use a lot more than I have. I could build more $5 billion accelerators. We'll probably get to that later (laughter). Astronomers are certainly underfunded. So are the geographers, the geologists and the seismologists. And I hope at some time we can get to some discussion as to how decisions can be made on what's happening to the direction of funding. Maybe somebody with more experience with the NSF than I have can tell me why it seems to have gone so far - not in adding its support for engineering, but in adding it at the expense of the support for what you might call the base beneath the engineering, the fundamental science.

#4 (again): In the view of the responsibilities of the chairman, I think some of the points you've brought up would take another five or ten minutes. I'd be happy to discuss it if allowed, but not now.

BS: I was going to suggest that after our break we should get into the discussion of priorities as the first topic.

#5: My name is Joel Yudken. I'm director of the Project on Funding Policy in Computer Science, for the Association for Computing Machinery. I want to suggest that perhaps one problem has to do with the political climate, the political environment we've been in especially over the last decade, in which research and development has been largely dominated by the priority of the arms build-up in this country. Now it seems to be moving in the direction of economic competitiveness begin the overall filter for what our R&D priorities should be. Within that, I feel that both the agencies and the scientific community operate in some ways, by necessity, in an opportunistic way. That is, if you look at how the budget process takes place - the way that the NSF budgets are decided upon, what the DARPA budgets are, and the other agencies budgets - these are decided upon quite differently. DARPA's budgets are separate and are part of the whole defense appropriations process that to some extent has been more advantageous to DARPA until fairly recently with the discussions of defense cutbacks. These may well affect their long-term situation. That's one of the reasons, I believe, that they have been moving to become more actively involved in the competitive issue.

The National Science Foundation is in a whole different category and is handled by a whole separate set of committees within the Senate and the House. And when their budgets are decided, it's often a trade-off between whether money is going to go to housing or other areas, as well as to go to science.

Bill Perry, who is former research director for the Department of Defense spoke at a banquet on Sunday. He made a point that I thought was interesting. He said that he feels that in Congress there is fundamental support for general science. But in terms of trying to get money for it, you really have to focus on specific projects. Because, when the Congresspeople vote on it, they start thinking in terms of "Is this going to be in my state? How many jobs are going to be created? How much money is going to go to universities in my state?" Where does my constituency stand on certain issues?" And that's the reality that ultimately is deciding where budgets are going.

I think the National Science Foundation, has been in an increasingly tight spot over the years. I believe that Bloch is perhaps responding to the pressures of saying "How are we going sell NSF and make it much more part of the future direction of U.S. science?" ... of noticing that DARPA's budget and other agencies' budgets compared to NSF's have gone up. And now he is saying that "We want a piece of the action." One way of doing this is with the science and technology centers. I think this is the motivation [for the S&T centers] as opposed to basic science, because you can't get the Congressional support for that.

A good example of this trade-off occurred a couple of years ago, when they were first pushing for the budget for the centers, I think they wanted a lot more. They requested a lot more money for their budgets. But Congress had to cut their budgets. That was two years ago. This year they got money and they were able to fund five [centers]. I think the initial plan was to fund a lot more than that, and maybe at a higher level. But I do think that this is the reality that's shaping how science budgets being directed right now.

RR: I think it's quite clear. And it's been made explicit publicly on several occasions that behind the proposals for the S&T centers of the National Science Foundation was that [motivation]: you couldn't double the size of the Foundation, which was the policy objective within a five year period, by promising to do twice as much as what the Foundation was already doing. There simply was not a political constituency for that. It was necessary to "package" the product differently, to link it to an important national concern and to demonstrate that there was a new way of attacking the problem in the expectation, I should say, that much of what went on, what would go on in the centers, is the kind of what have gone one if additional funding had been made available in the investigator initiated projects, in compliance with the budget.

That may have been a feckless calculation. I don't know. It really is too soon to judge. I think there are some very important and serious questions of the vulnerability of these centers to political influence that were raised earlier that we are going to have to be very careful to deal with in the future.

#6: I am Jim Hayes, National Science Foundation. I can't respond to all the comments made this morning. But I do want to speak to a couple of points. One, the percentage of the Foundation budget that goes to individual investigators in science is over sixty percent. That percentage has not changed very much through the years. The absolute amount in dollars has been going up. Not as fast as it should have been, but it's been going up. The part of the Foundation's budget that has been going up fastest is the science and engineering education budget, which several of you spoke to earlier. With respect to the 8% of the budgets that goes to centers, that includes science and technology centers, engineering research centers, materials research laboratories, plant science centers, other kinds of centers. In each of those cases the Foundation did not specify certain fields where it wanted to do research. The fields were chosen by the institutions themselves; in the proposals that came they proposed their own subject matter and their own ideas for the kinds of research to be carried out in those centers.

RK: I think that may be part of the difficulty. Because, in the case of the engineering research centers in the end a blue-ribbon committee had to choose between, for example, solid-waste management and signal processing. The charter was so extremely broad that no small group could have had the necessary expertise. Also, it seems that the review process was somewhat faulty. After extensive preliminary screening, as I understand it, there were twenty proposals still surviving. Each of those then had an opportunity to present its merits for about twenty minutes to the blue-ribbon panel. And these proposals spanned an enormous range of topics. And then the blue-ribbon panel very rapidly made its decisions. And these have been widely criticized. So I wonder why it was structured this way?

#6 (again): That's not correct. The review process, the three hundred and some odd proposals that came in, went out for peer review to expert reviewers within the disciplines. The first round of panels consisted of panelists who were expert in the particular disciplines that were grouped in related packages.... all the superconductivity ones together, the computer science ones together. The result of that was about fifty that were chosen for site visits. The site visits were made last summer by teams that consisted of the NSF staff, outside people who were expert in the field, plus people from other fields. Essentially all of the institutions that were site visited had already received high marks for the quality of the science in the area in which they were covered. What the site visit teams looked for specifically were the coherence of the center. Was the center an improvement over what the same amount of funding in the form of individual grants could do? The blue-ribbon panel then looked at the results of the site visits. The centers themselves did not present their cases. Their cases were presented by NSF staff. The decisions in that last stage for the most part were not decisions on the quality of the science, but on the nature of the centers themselves, and the distribution of a field.

RK: I may have been wrong in detail. But I submit that what you said more or less confirms the picture that there was a certain amount of preliminary screening, and that that was done responsibly. But then at the end factors that one could argue should have been secondary prevailed in the final selection.

#6: The factors that you are talking about are factors that go into the decision on every NSF proposals. Effect on the infrastructure of science and engineering is always a factor.

PA: It seemed to me that NSF has often argued that the overwhelming response to some of these proposals was a good argument in favor of them. It's worth saying here, although we all know it, that the great majority of scientists would rather have the individual grant than their participation in the center. They feel that participation in such a proposal is a necessary evil that's visited upon them by the organization of science funding. Certainly many people were disturbed by the emphasis on infrastructure questions. The questions that were asked at the site visits left many people very much disturbed.

Someone else from the floor: I'd like to comment that I don't think we are really addressing the question by NSF bashing (others around him say "Thank You"). Given the circumstances, and the sort of the maelstrom around them, they do a very honest job. It may not be altogether correct in the view of each of us who depends on the NSF. The point is we should look at the overall problem, but this is a place where it's hurting. It's not going to be cured entirely within the National Science Foundation. We have to look at the broader issues that affect the overall support. And some of these have been mentioned. The rationale support, and so on. I did want to say that.

BREAK

BS: During the break Rolf Sinclair approached me, and said that he was very distressed that this has turned into an NSF bashing panel. So I suggest that we try to do some other bashing as well (laughter).

Panelist?: I'd just like it noted for the record that I don't think I've bash~~ing~~ed NSF.

Floor: If you bash us, at least get it right when you bash. I'd be perfectly happy to take all those bashings...

BS: I'd like to suggest that it might be a little like family feuding. You are probably more critical of the people you are closest to.

I'd like to open with a question that I hope will move the thrust of the discussion away from the NSF, at least temporarily. As I said we'll start with priorities now, and I'd like to address the issue of who is determining priorities and why. We all know... as I said, to switch the bashing...that the DOD is the dominant actor in all of R&D in the United States. The question I'd like to pose to the panelists is are priorities set with weapons development in mind? Are those sciences being funded that the government, the Congress, and the DOD feel are the most likely to contribute to weapons development? If this is not the reason, then what are the issues and priorities?

BR: Can I start to differ with the moderator? I don't believe that the DOD is the primary source of R. It may be the primary source of D. But R is much more strongly funded outside the DOD and the DOD is a very minor player. DOE, National Institutes of Health, National Science Foundation, those are the places that are supporting the R. DOD certainly has a collection of priorities, but they mostly go to the questions of engineering development, and so forth, and not to the basic part of research.

RP: The one exception is probably computer science...

BR: Yes...

WT: Even in mathematics, approximately half the money comes from DOD - DOD plus NSA.

RK: I think that what's happening is that the universities are doing more D and less R, largely because the DOD is pouring increasing amounts of money in.

BR: I think at Stanford, DOD is fourth in support of work at Stanford. And I don't remember the order of them all. But I believe the NIH, DOE and NIH are all bigger than DOD support of R&D at Stanford University. I don't know how it is at other universities.

WT: We are talking about the physical sciences here. The NIH by itself sort of dominates everything else.

BR: OK. Then if I drop those and I just take the physical sciences, then DOE and NSF are bigger than DOD is at Stanford.

RK: In the department of electrical engineering and computer sciences at Berkeley, upward of 70% of the funding comes from DOD, and about 10% comes from the NSF.

RR: Well we know what the numbers are actually. At the present time, roughly 70% of total R&D, civilian and military, is funded by the DOD, 30% elsewhere. That ratio has shifted dramatically in the last 8 years. In the late 70s, 1980 roughly, it was about 60% non-DOD and 40% DOD. So a major shift in the allocation of funding has occurred in funding in the last decade. And the major question in my mind for the next decade - for the next half-decade - is how to shift back to what seems to be an altogether healthier relationship between military and civilian funding, or between military and civilian purposes. And that's going to be a neat trick. But I think it's one that we have to pull off. Because it's the only way the priorities issue can be helped.

RP

~~RA~~. I think that's very important even for the Department of Defense. I made a slight reference to that earlier. Somebody mentioned the fact that the Army is now supporting research in high-definition TV. I don't normally do spin-control for the Department of Defense, but as a matter of fact I think that particular program is hopelessly too late, too small, and doomed to failure. But nonetheless, it represents some sort of belated recognition by the Defense Department that it's dependent on the rest of what's going on - that it's technologies come from outside, not from inside. There is no doubt, as I think Burt indicated, that at least in our sciences at the universities the Defense Department's become a minor player in recent years. And the work they're supporting... I would even go a step beyond that...the work that the Defense Department is supporting in the department of physics at the University of Maryland is to some extent a less interesting, much more applied development sort of work.

WT: I wanted to mention E, too. One of the main functions of universities is education. I think scientists, mathematicians should view education as one of their main goals. I think that one of the bad effects of the very high emphasis on development is that education has become a very minor part of a lot people's view of their job within the university. I'd like to see more action by scientists and mathematicians with the goal of improving education. Maybe one way to help fix the system would be to go back to a funding system where education is one of the main rationales for maintaining the university system.

BS: I think this gets us to the question of what we should be doing, which has been discussed quite a bit recently, especially starting with the speech Frank Press, the President of the National Academy of Science, gave last Spring. In it he suggested that the scientific community, especially the National Academies, get involved in making decisions about priority for the budgets that are being dealt with by the Congress. I'd like to know how the panelist feel about that, whether or not they feel this is the appropriate vehicle, and whether or not they see problems with the National Academy of Science, the National Academy of Engineering and the Institute of Medicine being basically the sole decision makers in this arena? Or should we be trying to democratize this situation a little bit by trying to involve other scientists and people who are not members of the academies.

RR: Let's be clear about how the systems works. Because the way you described it, I think, is somewhat misleading. We are talking about the public's treasure. The public's treasure is disposed of by the public's elected representatives. The President submits a budget and the Congress acts on it in the form of a budget resolution and appropriations. So the decisions about where money is going to be spent for science, and how much will be spent for science will be made in the political arena. Now, the political arena in our country is not a closed one. It's open to inputs from lots of places, including the scientific community. So the question we have to deal with, I think, is "Should the scientific community undertake to assist the political decision makers in their decisions by better informing them than they would be inform in absence of scientific input?" Or is the problem so difficult, so internally divisive, so intrinsically impossible to judge on rational meritocratic grounds, that decisions are better left to the decision making devices that the political arena has for dealing with these questions?

Nobody has asked the National Academy to decide how money should be spent for science. It would be wholly inappropriate for them to undertake to do that. But it seems to me entirely appropriate for them to do what they have done, started to do, which is to set out some principles of allocations, some categories of allocation, some ways of thinking about how science funding ought to made, that will enable political decision makers to do their job better. I think the politicians can do a better job of organizing themselves for that tasks. And the key to that is to have a Presidential Science Advisor who is in fact close to the President, who has the President's ear, and who is deeply involved in budget decisions. This has not been the case for at least the last eight years. It's only at that level, it seems to me, with a Science Advisor who has the confidence of the scientific and technological communities, who has the confidence of the President, and who has a voice in what actually is going to happen, that the scientific community can then be brought to the table and talk realistically and in practical terms about what's most important, and what is deferrable.

PA: I think it would be interesting to have the panel discuss the question of what Frank Press said yesterday at his press conference, that 95% of the scientific public certainly rejects the idea of either a cabinet department or of a science czar. I wonder if that's the case, whether it's 95% of the panel. The advantage of course is that we would have an undersecretary of science at this point. It would also mean that there would be at least somebody at that level, and some committee of Congress whose job would be to to get as much money as possible for the overall science. The fears of course are obvious. The fear is that then all the other agencies gang up on him. Another fear is that you have a person that has too much power. I don't see that it's worse to have person with the visibility of a cabinet officer than it is to have a science advisor who may or may not be someone that the science community as a whole would like to have. I'd like to hear people discuss that.

RP: I don't if the percentage that's quoted is right. The people I know have a good deal of misgivings about a czar of science. I agree that while we have to be concerned with the whole business of setting priorities, we have to be concerned with the problems that this raises. They are a different sort of problem than we've been confronting. In the first place there is a risk that we may institutionalize the zero-sum game by focusing attention on how we divide the pie up, rather than on what size the pie is. And I think there is a real danger, and to pit one against the other makes absolutely no sense at all. Those are the decisions that in the end our elected representatives have to make. I don't think science can add anything to that. So the question is, at what point do we set priorities? And I believe that we have to address that very carefully.

WT: I don't see why scientists couldn't help set priorities between things like the SSC and the human genome mapping project. There's some question of what fundamental understanding you are likely achieve with these two different projects. I don't see that people in Congress are necessarily well-equipped to make that kind of decision without at least a lot of advices. To me as a mathematician, not involved in either one, something like the genome mapping is a much more fundamental thing than the SSC. It's not clear to me that either of these projects is kind of ripe for doing right now. The technology for gene mapping is improving so quickly that the cost of it will exponentially decrease with time. Maybe so also with the collider. Perhaps if we didn't rush to put so much money into it now, we could solve the problem much more cheaply in a few years.

BR: On the question of the science czar, I completely favor such a science czar, as long as the czar is me. And that I think is the trouble with the science czar and the whole notion of a Dept. of Science. As far as your comment, Bill, on the human genome and SSC, what you have made is a judgement based on popular science. Because you don't really know a hell of a lot about either one of them. And that is the kind of judgement that our politicians do better than you or better than me, I believe. I think that it is the responsibility of the scientists in a given area of science to get their own priorities straightened out and to decide what's most important to the advancement of their discipline. Once that's done, I think we should be trying to advance our discipline, each of us trying to do it. I think that we have a rather good mechanism now. We have a mechanism where various agencies have first line responsibilities for different areas. And you try to convince them, and through them the administration, to fund whatever it is you want to do, whether it's more mathematics, fellowships, sequencing the human genome, building a huge accelerator. And we have a system where our Congress is going to make up its mind on whatever is proposed by the administration. And it make up its mind on the basis of hearing a lot. It sees a lot in the press, it gets a lot of advice from scientists, and it gets a lot of lobbying. I think that kind of lobbying is perfectly appropriate.

I don't see how a system where scientists try to decide priorities between the fields can work. We have seen that other places in the world. And it doesn't work. That's basically what the British do with the Science and Engineering Research Council. The government give a pot of money to the advisory board of the Research Council, and it gets parcelled out. And what happens is that each research council now starts to divide the money into even chunks. And whenever one thing gets to be too big, it get cuts back so that it is the same size as all the others. Right now, for example, you can look at Britain in radio astronomy. Britain was the place where radio astronomy really went big time. But it got too big, and so it got cut back. And now Britain is not a big player. Britain also had the first dedicated synchrotron light source, but it's too expensive to run it 24 hours a day, 7 days a week. It would have required an unequal division of the budget within the Science and Engineering Research Council. So it was run 5 days a week, for one shift at a time, and nobody has every heard of anything that's been done with it. I think scientists are really rotten at deciding priorities between sciences, because I'm in my field because it's wonderful, and you are in yours because you think it's wonderful. I think mathematics is a useful tool for our theorists, but I don't think it's important other than that...(laughter)....in(??) physics, if people who develop new theories of strings and topologies, may be important for mathematics. But that's about all.

And so I think each of us has such a strong bias, such a strong belief in the importance of our own area of work - after all, we've committed our whole careers to it - that we are not good judges of what should be done in broad areas. I think that we have a system which is extremely imperfect as hell. There's a lot wrong with it. But basically, it's right. The scientists propose. You argue with people. You try to convince them. You try to sell your notion. And sometimes you succeed. And science has been growing as a result of that system. I don't see what there is to replace it. It certainly to my mind not a collection of scientists trying to decide between fields.

BS: I take it that you don't agree with Frank Press.

BR: I never read Frank's speech, so I don't know what he actually said. I don't believe that he said... I can't believe that he said that the National Academy should recommend specific budgets to Congress. What he probably said is that the National Academy might be able to help on telling them what fields are really ripe to push. That's perfectly OK. It can tell them what to push. Maybe I should try and get that speech from Frank, and find out what was really in it.

RK: I was wondering if the decision making process, and the process of advising Congress could be improved if there were more mechanisms between agencies. It seems that each agency is operating in its own environment, with a different set of competitors for funding, and a number of items fall between the cracks. Recently, an interagency organization called FCCSET has been established. It seems to have made some steps towards exchange of information. As a result some new programs have been funded. I was wondering what the panel things about improving that kind of communication between the agencies?

RR: I guess I think it can always be improved. It's never as good as it ought to be. There is no particular reason to believe that it's working as well now as it should. So I think that that's a promising avenue to explore. I want to comment on Burt's observations, because I believe with him that we have developed a marvelous system. In fact, I believe that the system we've developed in science is one of America's great contributions to statecraft in the world. It really works very well. Elected representatives make large decisions, and they are the ones best able to make it. But there is nothing in the system that says they have to make them as uninformed as possible. Even more fundamentally I think you have to agree that what's made the system work as well as it has is that every year until very recently there has been more money than there was before. You lubricate a lot of problems, a lot of problems, by having money to spread around.

My concern is that we appear to be going into a period in which that may not be true. Certainly in terms of the whole federal budget, that is not true. The rate of growth is going to slow dramatically, one expects, unless new sources of revenue are found. And so there is going to be less money to go around for science in a period in which their appears to be more science to do - more useful science to do.

What do we do about that problem? Well, I think that elected decision makers need help in how to think about it. Not what to do, but how to think about it. Because it really is a new problem in our experience. We have not had much experience in thinking about how to defer something in order to do something else which is pressing. These are very difficult decisions. Left to their own devices, I can assure you that the politicians will do it the way they are accustomed to doing it. And that's a system that depends heavily on trading, on constituency advantage, and on other elements that are certainly important, that are intrinsic to the democratic process, but that are not themselves sufficient, I believe, to take into account the best interests of science and the best interests of the nation. So I think that Frank ought to be congratulated for starting to try to think about that problem, which is not to say that he has found the right answer. I don't know what the right answers are. I have some differences with him; I agree with him on some things. But at least the conversation has started. And it is an absolutely critical conversation to pursue.

PA: Burt's remarks remind me of the poker game at midnight. There's one guy at one side of the table saying, "Have I told you the story of the farmer's daughter and the salesman," while taking a long drink. And on the other side of the table is a guy saying "deal". Burt is on one side; he is the winner who is very happy with the system it exist. Computer science, mathematics, and solid state physics have been the losers in this process, and we would like to see some new way to get the hands dealt in some different way. Computer scientists are desperately dependent on DOD funding, which is a very unhealthy situation. We've become desperately dependent on no funding, which is also not a very healthy situation.

So, it does seem that there are some very good reasons why we have to think about that organization. I would like to know how to do this. Somebody came up to me - Gunsalus ~~Gonzales~~, in fact, came up to me during the interval - and said that we should be really touching more on this question: Is the way to fund new forms of research, ~~in (??) the~~ inter disciplinary kinds of research, these centers, or is it not? There's a whole world out there. People who think that the way to solve all these problems is by changing the game by introducing interdisciplinary centers or whatever you want to call it. Rather than by trying to reprioritize the funding as it exists. I'd like to hear people's comments about that.

WT: Is there someone in favor of it?

RR: There must be somebody because that's the way money is being spent.

PA: I was told that there was a whole symposium yesterday, that was very strongly in favor of centers, especially interdisciplinary.

BR: (??coughing) somebody trying to do it a better way than we have been doing it now. By the way, as far as being a winner, I'm glad you think we are such a winner. The peak of high energy physics was in 1966 in inflation corrected dollars. And I think this year it is going to be back to what it was in 1966. But, in thinking about problems, I believe that your area of physics has a special problem. It is the most diverse and incoherent community that I know of. It is much easier for the particle physicists to get a coherent priority list together. It is much easier for the nuclear physicist to do it. It is much easier for the astronomers to do it. But when we get into your area of physics, there are so many different subdisciplines mixed up in it, that there has been, as far as I can tell, no successful attempt to try and set internal priorities. The DOE is trying something, called the Basic Energy Sciences Advisory Committee, which is a group of scientists from different subdisciplines in your area. And maybe they can, after a few years, hammer out a coherent program. Without a coherent program in various areas of science I don't see how we can do a lot to enhance your part of science. And I agree with you that you need more money. The multi-disicplinary centers are, I think, an attempt by Erich Bloch to give the whole business more pizzazz, to make some sort of statement that the whole is going to be a lot more than the sum of its parts, and therefore we should give more money. I remember when the NSF tried this once before. In the late 60s they had a program called RANN, Research Appropriate to National Needs. And they had centers of excellence. And all of them disappeared without a trace. Nobody knows anything that's come out of those programs. Maybe something better will come out of these programs. I think something that is really very critical is to try and get people in materials, atomic, and solid-state areas to lay out a coherent collection of priorities that's more than just a statement that "we need more money for individual researchers."

PA: We have done that for big facilities. I was on the BES committee [illegible] I admit that we have not done that for the field as a whole. For big facilities our recommendations have been honored in the breach. We gave the major priority to neutron scattering at least twice. We've never gotten our neutron scattering.

RP: I'd like to elaborate a little bit on this. The request came from the President's Science Advisor to the the Academy to convene a committee to set priorities for major facilities in the solid-state sciences. And that committee was formed. It came it out with a list. Obviously nobody agrees with the list completely. But I think they did a credible job. They came out with a defensible list. It was turned over to Dr. Keyworth, and they promptly began to recommend in inverse order.

BS: That brings us to another question. Who holds the power in making these funding decisions, given that mission-oriented agencies play an important role? I'm speaking not only of DOD, but also of DOE. Do the funding agents hold too much power, too little power, or just enough power on making decisions about which areas to fund and with what amounts of money?

WT: I think there is a certain fallacy in this idea that a discipline should set priorities, that it should have a plan that the funding agencies would hopefully implement. I think we have too much of this atmosphere of planned economy in science. I think science, at least mathematics, is inherently unpredictable. There are thousands of different interesting research projects going on. Most of them don't amount to something highly visible very soon, but my experience is that you can never tell where the most important advances are going to come. I think that not only is it not very constructive, it seems to actually be destructive, when you try to set priorities among these highly diverse endeavors.

PA: One very brief remark. I want to say that whether they have too much power or not, I will unbash the National Science Foundation and say that the individual project officers have in general performed much better than one can possibly expect.

RK: I think that the NSF is the bastion of the kind of science support that any of us wants. I think scientists themselves believe in general that in any subfield you can identify where excellent work is being done. One of the priorities, if you will, should be to make sure that that excellent work is supported. But I think it's very hard to sell higher up the idea that you should support work just because it's good. I think the scientists believe that. But that doesn't seem to be the way to sell it any higher. I think that's unfortunate. I do think that there's an issue one level below the political process. You get to the implementation, you have the various heads of agencies, deputy directors for research, and so forth, making more detailed decisions. There you have a tremendous concentration of power that is a little bit frightening. I think a small number [of people] at DARPA, for example, with a fairly inbred collection of advisors is making overarching decisions which influence the whole development of, for example, computer science. And I think even in NSF things can go that way. You begin to get czars in areas. I know that this has been the case in computer engineering, for example, where in previous times NSF was putting more emphasis on funding proposals generated by the scientists themselves. So, although both of those agencies have on the whole done a good job, I'm really concerned about this concentration of power.

RP: I think that's exacerbated in the present climate, at least in the areas that Phil and I are concerned about. There is a very large number of good proposals that are not funded. And when you have that situation, while I agree with Phil that the project managers have done a superb job, the fact is that they have a group of proposals, all of which are highly ranked. But they can't all be funded because there isn't enough money to go around. There comes a point in which the peer review system isn't functioning anymore. Because, the percentage being funded is just too small to make any sense. And I think we are kind of in that category right now. I think it poses a risk for the peer review system. People keep telling us that scientists have to bite the bullet and make hard choices. We've been making hard choices for years. We call it reviewing proposals. When we see work that we think is not good, we give it a bad recommendation. It doesn't get funded. I think that's what making hard choices is. We are not unused to making hard choices. It's just that we have to look at the problem more broadly now.

BS: I would like to ask the panelists what specific recommendations they can think of it to try to correct some of the problems we've been discussing.

RK: I'd like to repeat one suggestion that I've made in a different context earlier. And that is to dispense more of the money to the best students, to the best post-docs, the best departments, rather than to investigators in specific research grants. That way you get a more pluralistic system. Give the money to the top students. University departments compete for them. They are going to know who the lively, interesting professors are. And, I think in that way, intellectual strength might become a larger component in the allocation of support.

PA: It is interesting that the same proposal was in the paper I prepared for this panel. But it is essential to have some quality control. There are a certain number, who have to remain nameless, of glamorous but not quite sound professors even in our better universities. There has to be some quality control. I have seen that system operate in England, and it operated very well. It's one of the stronger things in the English system, where a large part of the funding goes to the students.

WT: What mechanism would you recommend for that quality control?

PA: I think it can be done by departments, or possibly by the agencies. I think that the program of the students would have to approved by the agency.

BR: That sounds like setting up a more complicated bureaucracy than the one we've already got.

PA: I don't think it need be very complicated.

BR: I would personally like to see longer grants and contracts. That would get rid of a large fraction of the problem. We should be talking about three to five year grants and contracts that should be for an area. They should start with what you are going to do first, specifically. But they shouldn't chain you too narrowly beyond that, because scientists should be able to follow their nose. Good scientists, the best scientists are the ones with the best noses for problems. And so if you could get a larger percentage of long-term contracts that would not require too specific a description of what you are going to do, I think you get away from what some people complained about, about having to lie in your proposal about what you want to do. That leaves a problem of how new people are going to break into the field. New people may not have the track record to have the agencies calmly hand them the money for five years worth of work. So maybe you should keep the shorter ones for new people just starting out. But as they are getting established then their contracts ought to be lengthened too. It's a very tough problem. I think all we can try and think about are gimmicks that give the scientists more discretion. But still keep them reviewed by their peers, but not reviewed so often, and not having to spend such a big fraction of their time chasing money.

RR: On the question of graduate support, I think it's true that we could use more grant support directly to students in the form of competitive programs in which the best students are chosen for support. The National Science Foundation has certainly been moving that direction. It's been doubling the size of its fellowship program. There is a new programs authorized and funded in the Defense Department that probably will be run in the same way. The National Institutes of Health need more money for that purpose. Those programs are important, both because they make a symbolic statement as to what the society values - if it values high intellectual achievement in science it's going to award it - and because they can be helpful as counter-cyclical labor market devices - a labor market in which there are strong incentives for undergraduates to do something other than go onto graduate school and a research career. These programs can intervene at a critical time and move them in a direction that we think is desirable. But if that were to become the dominant method of supporting graduate students in the sciences, I think it would be very unfortunate. It would seriously undermine the whole notion of apprenticeship as the basis for graduate education in the sciences, not to mention the damage it would do to the conduct of research, although I suppose researchers would figure out a way to deal with that one way or another. I just feel that direct student grant support is an essential, relatively small element of the total student support system. It ought to be made a little larger. The consequences of making it a lot larger could well be to produce a system of graduate education and a research system that would be different in ways that we wouldn't like at all.

RK: I think there are still a number of mechanisms that allow the faculty to supervise what the students do. They have to find a research advisor, after all. In the end they have to file their dissertations. So I don't think that things would fly off in all directions.

RR: I can tell you in the fields of the social sciences and the humanities in which, in effect, students are independent agents, because the grant support doesn't exist, the notion of the faculty member as a close research mentor is one of the nicer fictions.

WT: But is that because of the structure of the discipline itself, or the structure of funding?

RR: It's hard to separate the two out.

WT: In mathematics, there is a certain amount of graduate student funding through grants. But I think that in the overall picture it's rather small compared to the support of graduate students through teaching assistantship. In most departments that I've seen graduate student support through the NSF tends to be pooled. We have a certain amount of graduate student support on our grants, but then it's all administered through the department. Still, there's a very close relationship between the graduate students and the thesis advisor. It's essential, and it's based on the fact that they have to learn from us.

PA: Certainly, when I was in England I saw no problem with the relation between the professor and the students.

RK: I think the only thing that might go wrong is that some ill-conceived large projects might fail to get the bodies they need in order to operate. In my own field of theoretical computer science it works pretty well as it does in mathematics. The students have an enormous amount of latitude. It's a fundamental principle that they should. And we learn a lot from them, because they investigate the fields that they care about rather than what we have determined. But the relationships work out just fine.

BS: The last few minutes we should open up the floor to questions.

#1a: There's been talk about priority setting in using the President's science advisors - excuse me, I'm Jeff (???) from The Scientist - on using the Science Advisor as a mechanism. But there is also a lot of talk about the difference between various agencies, and how they proceed through Congress, and how they set priorities, on the range from DOE to NSF, etc.. I wonder if that diversity mitigates against the hope that there can be any kind of overarching cross-cutting guidance, whether it's the Presidential Science Advisor, or the Academy, or any other kind of system, given the diversity within the federal government, how the decisions are actually made, and how the money is actually parcelled out.

RR: It certainly limits those possibilities. And I think on the whole, properly so. I would be unhappy with a system in which it were possible every year to decide what everybody ought to be doing, to put the money where somebody decides it ought to be. But I don't think it's unreasonable, in fact I think it's terribly important, to increase the sense of order and discipline in those decisions. I'd settle for 5%. I think it would be a major achievement, rather than a kind of law of the jungle that's been operating in recent years.

RP: But before we get a science czar, we need to get a science advisor. We seem to have trouble even doing that.

PA: I think that not enough has been made of the rather small amounts of money, the relatively small amounts of money that are involved in some of these things. You can fix the problems in computer science without many tens of millions of dollars, perhaps less than that. You can fix the problems of physics - $10 million is a good estimate. These are tiny amounts of money relative to the overall government science budget. And we have some very squeaky wheels here. The Science Advisor could at least say that it doesn't take much oil to fix that squeaky ~~leak~~. wheel.

#1a(?): If I can follow up on what Robert Park said, given you don't have a Science Advisor, and given you have a DOD, you are talking about shifting the balance from military to civilian. Doesn't that give you some indication of what you are up against in the real world, and how possible it is to actually move the elephant?

RK: That's the question...Do you start within the existing structure, with DARPA becoming the MITI of the United States, or do you reject that because it would work against the long-term more rational solution.

WT: I think that scientists are very good at generating bull-shit, because they need to at times to get a grant. But I don't see why scientists couldn't be more persuasive if they said what they really believe about why we need to support science, rather than taking the easy road of military funding.

#3a(?). Can I make a couple of comments? It's interesting that the issue of priorities has been discussed for several minutes now and nobody has defined what it means to set priorities. Are you trying to make value judgements between areas of science . As Burt pointed out, we each believe we are doing what we are doing because it's very important. To give some definiteness to it, though, I have heard that an operationally possible view of priorities is that of timing. And indeed, that couples with the view that Dr. Rosenzweig expressed. I will put it in a slightly different way. We have nothing to lose by providing better information to people who will make the decisions. And those people who will make the decision must be the people that are the keepers of the public's funding.

PA: One could ask if it were possible to develop science priorities and a sense of what really goes into priorities. Can one can do that without really setting priorities? One aspect that is very seldom mentioned relative to other fields of science is how many cables are their from this particular field of science to other parts of science? Certainly, one doesn't want a citation index type of study. But one would like to know whether a given development is relevant to only the people who think it's the most wonderful thing in the world, or whether a lot of other people find it a very interesting thing to think about and something that can be used in the world. I think that kind of thing is what really gets lost.

RR: Let me give you a specific example of a kind of decision that troubles me. There was a piece in yesterday's [San Francisco] Chronicle, by Randy Shils, a writer who has written a lot on the subject of AIDS, sharply critical, bitterly critical of the administration's 1990 budget because it proposes only a 25% increase in the funding for ~~aides~~ AIDS. It was also sharply critical of biomedical scientists and people in the research establishment for not speaking up publicly and complaining about how limited that increase was and inadequate it was to do the job. The article closed with this statistic: "Since the first AIDS case was discovered, 81,000 cases of AIDS have been reported, and 48,000 plus or minus people have died of AIDS." That was on page three or four, something like that. On page six or seven there was a report from the meeting of the American Cardiological Association that is going on down in Monterey right now, which reported that in the last year 975,000 people have died of heart disease. That number is coming down, but it is still very large. The article could have said that the funding for heart disease research in the National Institutes of Health has decreased. One of the reasons it has decreased is because the funding for AIDS research has gone up so sharply. There ought to be a way to bring those things together. I understand that there is a political constituency for AIDS research. I understand that there is an urgency to it. But there ought to be a way to bring those things together so that members of Congress, whose minds are open on the subject and are genuinely trying to figure out what's the best thing to do will have some better structure in making those decisions than they now have. I think one can find similar instances in many fields, although not as dramatic.

BS: One last question.

#4a: I'm Cherrill Spencer, and I am a physicist at the Stanford Linear Accelerator Center. I think this question of what criteria are going to be required for setting priorities is probably the most important issue we can touch upon today. I feel that it behooves us, the scientists, to define these criteria, or other people are going to do it for us, and we are going to regret it.

I recall an editorial in the Christian Science Monitor that reported on, I believe, and OMB report, that was counting the number of graduate students in different disciplines, and relating it to how much money they gave those disciplines. The article noted that the number of graduate students in elementary particle physics was a whole lot less than you would expect for the money that was going to elementary particle physics. If we can't decide what those criteria are ourselves, then other people who don't know very much about the whole process of science are going to set those criteria for us. And it may be how many graduate students you've got. "We'll cut you down because you are not producing very many Ph.D's in your science."

RP: It's an interesting comment and I understand your concern. And yet I have not seen anyone sit down to create a list of what the criteria should be without that being one of the criteria. Now, it's certainly not the only criteria. And if other criteria are sufficiently persuasive, then that's taken into account. So that certainly has to be one of the criteria. As been pointed out earlier today, funding of academic science depends in large part on the fact that we train scientists. And we can't divorce it from that. Of course that has to be one of the criteria.

RK: I'm afraid I have to agree with Burt Richter that most of us are the prisoners of self interest, undoubtedly no better than laymen. Perhaps we are worse because of our vested interests at comparing our own priorities against those of other subfields of science.

#4a (again). The criteria that you must apply - that's what I'm talking about. We should all be able to decide on a common set of criteria.

RK: I don't think it's that simple. I don't think you can specify exactly how much weight the graduate student population should have. I think it's a complex puzzle. It's very difficult.

#4a(again). If we don't do it somebody else is going do it, and we are not going to like it.

RK (?): Those responsive to the public could perhaps do it better than we can.

WT: I've been thinking about the complicated question of setting priorities. It doesn't necessarily work in a logical, systematic fashion. I think there is an overall lack of discussion in the scientific community about what it is doing, and what's valuable, what's not. It's very clear to me that money is not being spent as efficiently as it could be. When I dabble with different disciplines, some of which I know better than on the level of popular science, there are certain ways you judge these matters [Missed some of the comments on the tape]. One of the criteria I think should intellectual diversity of a field, something you can't get very easily unless you talk a lot to scientists who are practicing that subject. I think one thing that the scientific community needs to do is have more cross-disciplinary discussions, such about high energy physics, the SSC, the human genome project. I think we can learn a lot and make the process of setting priorities much more rational if we have more organized discussions.

#5. I'd like to supply a bibliographical reference, if anybody is interested in criteria for priorities. The retiring address of Sir John Porter of the British Royal Society, I believe it was for 1987, will give you a scholarly analysis and description of some possible bases of setting priorities, which not only include the few that have been mentioned here, but many more. It's a very nice thing to read. And it will present you with great troubles....

BS: Thanks,...etc.....

December 19, 1988

HAS THE SCIENCE FUNDING SYSTEM BROKEN DOWN?

P.W. ANDERSON

Joseph Henry Laboratories of Physics
Jadwin Hall, Princeton University
Princeton, NJ 08544

Talk given at the AAAS Annual Meeting, San Francisco, January 14-19, 1989

I will discuss the thesis that the American funding system, specifically for small science in the universities, is in a state of virtual collapse, using what is probably the worst hit subject, condensed matter physics (CMP), as my example. The state of affairs in that subject is that literally dozens of academic openings exist across the country which are unfilled or not likely to be filled by qualified scientists. Many senior scientists' budgets have been eliminated or severely cut in spite of glowing peer reviews. The universities themselves have responded to a steady growth in interest among graduate students, and in perceived need for Ph.D. scientists by the economy, magnificently, but academic jobs are seen as hopeless dead ends because of the difficulty of obtaining research funding, and the likelihood of loss of that funding relatively early in a long research career. Another symptom of the general problem is the rapid turnover of program officers in this field specifically within NSF. Selfless young scientists accept that thankless job but are soon turned off by the hopelessly difficult decisions they have to make and, recently, by public abuse from NSF management when they do try to get the message across. Similar situations exist in a wide variety of fields of science: various engineering subjects, mathematics, growing branches of chemistry, biophysics, polymer science, economics, etc.

The difficulty is not total fraction of GNP of research spending in the country,—although that is a serious problem in itself. The total missing dollars are a negligible fraction of research spending, even if one were to add up all of the fields together. It is likely that in condensed matter physics at most $\$10^7$ are required to restore the health of that branch of physics. The increases that NSF alone has received under the Reagan administration, if applied thoughtfully, would have left academic science as healthy as it seems to have been a decade ago.

What have been the key causes of this problem? These are not necessarily all obvious, and they are worth analysing.

(1) A primary cause is simple bureaucratic inflexibility, especially within the NSF management. That management does not respond to changes in the intellectual structure of science. Some sciences have been productive of new ideas, interacted with productive sectors of the economy, and spun off new fields of intellectual endeavor, as opposed to other fields which are contracting intellectually or physically. Within physics the fields of nuclear physics and of experimental particle physics are contracting, whereas the general area of condensed matter physics, which has become much broader through its interaction with new fields such as pattern formation, neural networks, hydrodynamics, etc., has experienced enormous growth in the past decade, following on an explosive growth since the '50's. yet the division of NSF funds between these three fields has hardly changed in 3 decades. (If anyone doesn't believe my figures on relative change, he has not been talking to prospective graduate students in our major universities.)

(2) A second related cause is micromanagement and collectivization. A country which disapproves of collective farms and collective management of the economy seems very happy with collectivization of scientific research: the gathering of great almost unrelated lumps of scientific effort into Institutes, Centers (supercomputer and science and technology) and Laboratories (such as MRL's). There is a myth that modern science is more collective than it used to be, which to my mind, with the advent of computer mail, modems, user facilities, etc, is actually probably the reverse of the trend. What this does allow is micromanagement: the revolting "Thrust" concept of the MRL's being the worst example, where you fund something not because it is a good idea, and certainly not if it's original, but

because it fits your "thrust". You have to apologize and call it "seed money" if you do anything new. The centers themselves become political plums which can be delivered to appropriate Congressional districts without regard to scientific necessities.

A major effect of such centers is to remove the scientists from the front line of teaching. Graduate and undergraduate student selection and course work must be done within the academic department, and research should not be a separate career done somewhere else, but is best carried out as an integral part of education "in house". Every science department is desperate for science which can be done by advanced undergraduates and beginning graduate students as part of their education; yet the "Center" concept fits badly with this.

In general, some central facilities can be useful even in small science fields; the NSF's own National Magnet Lab, and Brookhaven's central scattering facilities, have been with us for many years and are essential. But many of the activities which go on in Institutes and Centers are perfectly well done in separate departments with students in attendance. And the user population which such centers depend on will dry up if the university science system continues to wither.

The STC program seems to me to have been no help, to say the least: again, we seem to opt for organised incompetence over generalized quality. There is no question, for instance, of the Illinois Superconductivity Center representing the best the country can do; it has very mixed quality, and does not have any useful contact with the major industrial players in this field (IBM, Bell Labs, Bellcore, Dupont): it seems to have been primarily designed for various political reasons, and represents a major transfer of funds from NSF's hard-pressed basic science budget to the DOE.

(3) A third serious problem has been the concentration of government funding in the Department of Defense. This has very many counterproductive aspects.

(*a*) Central DOD facilities may do "basic" research (though this is often mission-oriented towards economically nonsensical missions) but this research is insulated from the teaching process, especially by citizenship and secrecy requirements: nearly half of our students are now non-citizens and cannot work in many of the major laboratories, for instance.

(*b*) Quality control in the DOD is very weak; DARPA, in particular, has distributed its superconductivity money to places which I have heard described as "obscure", and I feel DARPA's track record to be miserable.

(*c*) Primarily, DOD contact with basic research in the university-industry-DOE lab complex, which is the heart of condensed matter research, in particular, is weak.

(*d*) "Basic Science" has a very different meaning to DOD than to NSF or the universities. DOD is a more reasonable source of funding for basic engineering, (but in fact wastes resources better focused on civilian projects) but only a pittance eventually reaches truly innovative basic science.

(4) The problems of the DOE would carry me too far afield. Here there is a regrettable identification with the nuclear and particle physics community which has only historical significance.

Can one suggest some remedies either within the system or outside it? I think there are some real possibilities.

(1) The National Science Board must cease to have a basically political and administrative cast. In the absence of a PSAC, it is probably the only body

which can function to advise the administration on scientific matters. It should have a majority of working scientists of proven competence and, in order to do so, can have no political criteria for appointment. The various science advisory committees of the NSF have been repeatedly ignored by successive Directors, especially Mr. Bloch, and unless they are given teeth they serve no function as far as I can see.

(2) The STC decision should be rescinded; in fact, growth in the NSF should, for a short period, be restricted to individual granting programs and to the basic science side.

(3) More visionary: there should be a separate budget for Big Science with a thoughtful setting of priorities at the national level for space vis a vis particle physics, or geological exploration, plasma physics, the genome, etc. These budgets should compete with each other, not each with its own small science component. NSF should be reserved for Small Science and user facilities, if possible.

(4) It would be a great boost to the smaller, more theoretical sciences, if, as in some other countries, a portion of funding went to the graduate and postdoctoral students rather than the professors. The students, by and large, know the most lively professors: within limits and with considerable control, a student's and especially a postdoc's choice of subject should not be totally dependent on whether the professor who intrigues him can get funding. In this sense, for instance, the ITP postdocs, the Junior fellowships at Harvard, and those at several industrial labs, are an excellent safety valve, and should be perhaps augmented with a national system of postdoctoral fellowships tenable at any institution acceptable to the granting agency.

Senator FORD. Dr. Anderson, I appreciate—I had several questions for you and in your capsule you answered them quite effectively and Representative Barton said that one third of our GNP came from technology initiated in high energy physics, and you took care of that right quick, so that ends that question. And then we were interested in spin-offs, and I see you have gotten that point taken care of, so you are moving right along here with me.

Dr. Hunter, Senator Bumpers I hope will be back in a minute. He was very concerned and interested and particularly had some questions for Dr. Anderson.

But, Dr. Hunter, in your budget book describing your requests for biological and environmental research—page 324, in fact—you indicated that no provision is made in the BER budget for $20 million of Congressionally initiated projects. In the description of energy sciences on page 411 you indicated that $39.6 million in Congressionally initiated projects are not funded in the BES budget. What are the projects you are talking about?

Dr. HUNTER. Which projects, sir?

Senator FORD. Yes, what projects are you talking about?

Dr. HUNTER. I have in front of me the projects. I can go through them. It looks like there are roughly 20 to 30 between the two categories.

What I would suggest is to submit them to you for the record.

Senator FORD. You certainly may.

[The information referred to follows:]

INSERT FOR THE RECORD

FY 1989 CONGRESSIONALLY FUNDED PROJECTS INCLUDED IN THE BIOLOGICAL AND ENVIRONMENTAL RESEARCH PROGRAM

Basic Science Building (The Oregon Health Sciences University, Portland, Oregon)

The Basic Science Building will consolidate the basic science departments of the School of Medicine, expand basic research space, upgrade educational space, provide additional dining space and improve site circulation. The facility will consist of approximately 130,000 gross square feet of space for laboratories, offices, teaching facilities and related support services.

Cancer Research Center (Medical University of South Carolina)

The Cancer Research Center is planned to provide state-of-the-art cancer treatment options; develop, through research, new cancer control strategies; and educate future generations of cancer specialists and scientists. The Center is planned to be a multi-story facility providing approximately 75,000 gross square feet of space to house research laboratories, patient treatment areas, and administrative and related support functions.

FY 1989 CONGRESSIONALLY FUNDED PROJECTS INCLUDED IN THE BASIC ENERGY SCIENCES PROGRAM

Center for Science and Engineering Technologies (Arizona State University)

The Center for Science and Engineering Technologies will provide facilities, equipment and services for interdisciplinary research in physical and life sciences with direct applications to new technology; innovations in high technology fields such as manufacturing techniques, computers, semi-conductors, telecommunications, airframes and artificial intelligence; and the accelerated development of graduate physicists, chemists, mathematicians, material scientists and computer scientists.

The proposed facility is a new multi-story building of approximately 101,500 gross square feet located in the midst of the existing science and engineering complex. The facility will provide research, education and administrative space.

The Center for Automation Technology (Drexel University)

The Center will be a state-of-the-art facility engaged in the conduct of basic engineering research on advanced concepts and systems, graduate and undergraduate education in the principles of automation and information technologies, and assistance in translating basic engineering research ideas for automation devices and systems into products for the market place. The new facility will be a multi-story building of approximately 54,000 gross square feet. It will house flexible laboratories, teaching space, and faculty/administrative offices and their support functions.

Center for Advanced Microstructures and Devices (Louisiana State University)

The purpose of the proposed Center for Advanced Microstructures and Devices is to conduct materials research bearing on advanced microstructures and devices, and to educate young scientists and engineers in the use of advanced materials research instrumentation.

The facility will include an electron storage ring and other materials science instrumentation, and approximately 31,000 square feet of building space for laboratories, experimental space, office and seminar rooms.

Proton-Beam Demonstration Cancer Treatment Center (Loma Linda University Medical Center)

The facility is a proton treatment center that will utilize a dedicated variable-energy proton synchrotron in a clinical setting for treatment of cancer and other diseases. The Center is designed primarily as an outpatient facility that will serve the Loma Linda University Medical Center and other local hospitals. The facility will be a regional and national resource for radiation therapy that will provide educational and research facilities for planning and development of other facilities.

The planned facility will include approximately 126,000 square feet containing space for patient examination and treatment, staff offices, laboratories, treatment planning computer work stations, and administrative and related support space. The facility will include a 70 to 250 MeV proton synchrotron, beam transport, beam lines, and safety and control systems.

Senator **Ford**. Can you give me a thumbnail sketch of why they are not being funded?

Dr. **Hunter**. These are projects that were Congressionally mandated; that is, added to the budget last year. Since these projects were proposed directly to the Congress by the respective institutions, we have not been involved in reviewing these projects. The Department of Energy, in order to maintain the quality of research throughout the different areas that we support, has a process that we go through that involves having all proposals peer-reviewed, prior to being funded, so we can ensure that the money is spent on the highest priority proposals.

That was not the case with these projects, and I believe the Administration's position was not to include them in this year's budget, sir, since they were not reviewed.

Senator **Ford**. Well, some of us are awful concerned about the peer review group that decides on what projects and what universities are able to get certain research programs, and we find out that this is a good old boy area and that good old boy just keeps moving into that research area up there, and they send them all to about ten universities.

And it is about time, and I think that is what Senator Conrad was talking about with Senator Gramm. He was against North Dakota getting a damn thing, but now he is in here wanting Texas to get it all, and so it becomes a political question that we are going to have to face one of these days. And the facing I want you to do is to give me in writing the reason that you are not funding these.

Just because a peer review did not come in and say to you they were good projects, somehow or another those of us on Capitol Hill that vote for it against what you request had some interest in them. I see a sore spot beginning to happen. Do you follow what I am saying, Doctor?

Dr. **Hunter**. Yes, sir, I understand. Let me just say directly to you that, since I have been in this office, I have made an effort, in any new initiatives that we have had to make sure that they were widely advertised to provide access to everyone, and we will do our best to review any ideas that are proposed to us purely on the basis of merit, Senator.

And I will be happy to try to take special account of the concerns that you have expressed, and we will be happy to supply the information you have asked for, sir.

Senator **Ford**. All right. We will look for you to include a list of those that are approved and where also.

In the SSC funding the support of the scientific community for the SSC in my opinion would drop off fast if it were funded by reducing the funding for the other science programs. You are asking for $250 million in fiscal year 1990. Your funding profile calls for $500 million for fiscal year 1990-1992, $700 million if you do not get non-federal contributions. That is your estimate.

Do you see any realistic way we can fund SSC at this level without taking from other science programs? Now is the time to let the committee know.

Dr. **Hunter**. Well, sir, we have made an effort in the Administration to keep the funding for the SSC separate from the other science programs. The other programs, such as condensed matter physics that has been so well discussed and presented here by Professor Anderson, are subjects of our support that has been growing.

We have a number of facilities' supported in these other areas at the very same time, which I alluded to in my statement, so I think that there is very adequate funding for the other areas as well. What we have is a situation where we have separated the SSC funding from the rest of the science budget. The Administration has handled it that way. It may have, in fact, made it too visible, for all I know, but it has handled it that way and we will not sacrifice, if you will, the other areas to fund the SSC.

If you recall in my statement the way I presented it was that we had criteria for maintaining scientific excellence and primacy in the basic research areas. We have to force the pace of research in these areas. We have to encourage excellence wherever we see it in order to attract new researchers. So, within the different areas, we followed those criteria as to which projects we bring forth. And that is how we basically formulated the budget request we brought to you, sir.

Senator FORD. Well, the $500 million that you are proposing here, the maintenance of the SSC will be about $300 million a year. That is your estimate.

Dr. HUNTER. Yes, sir, once it is constructed.

Senator FORD. And that is going to be outside of your basic research that you are now funding because we are talking about when President Reagan submitted his budget, as I said in my opening statement, that it would be funded from new money and then you hear Senator Conrad and his play with Senator Gramm, who is absolutely opposed to any new money, and he gets this $1.1 trillion budget and says you ought to be able to find enough under that. And he cuts the program in North Dakota so Texas can have it. That is when I get back to the Congressionally initiated funding that we got into and we are going to get into deeper, I am afraid.

Dr. HUNTER. Well, again, sir, let me just comment on that. The object of our undertaking the SSC is to ensure that we can maintain forefront basic research in the area of high energy physics. The $300 million number you referred to for operational expenses would be for operation of the facility after it is built. Let me just comment that we recently had the High Energy Physics Advisory Panel (HEPAP), and we will be happy to send their report to you, sir, look at the issue of how the SSC interacted with the rest of the high energy physics community. We have not yet gone through a budgeting exercise in the five to ten out-year period while SSC is built, but we expect since it will be the best instrument available, that people will become interested in working on that facility. So we expect some adjustment in the very long term, between the rest of the high energy physics budget and the SSC.

So, I would provide you the HEPAP report [1] that has to do with how SSC interacts with the rest of the high energy physics program in particular, and I would also say that we have gone to great pains in this budget, sir, to try to ensure that the other areas, such as condensed matter physics, are receiving big facilities and little facilities as appropriate. We are making sure that we can forward those, and we have increases in the budget for those areas as well, sir.

---

[1] The report has been retained in subcommittee files.

Senator FORD. Now, let us just take your statement one step further. That just takes us to an appropriations bill that is over budget. Will the President sign it under those circumstances?

Dr. HUNTER. Senator Ford, I have to tell you at this point that I do not have any way of being able to authoritatively answer the kinds of questions that were posed between Senator Conrad and Senator Gramm.

Senator FORD. Well, I am asking you if the President—and you are here representing the administration—if we take this budget and we take this funding without any new money, that puts us over budget. Will the President sign it in your opinion?

Dr. HUNTER. Sir, I do not have an opinion on that. I think that you have had forwarded to you a budget from the Bush Administration in which they chose to invest strongly in science and basic research, to continue the tradition of the Reagan Administration. To invest in that is a way to help us in our overall situation and to help ensure America's future.

To increase those amounts involved, they presented to you a budget regarding the rest of the matters with which I am not familiar in detail, sir. I would only say that the Office of Management and Budget would be the appropriate people to deal with on that. Within the targets that were established for us and the allocations and the overall priorities, such as the emphasis for research and basic science in this country, we have prepared a budget that has gone through the priorities and been forwarded to you, a budget in which, the different areas we support that we can ensure that good research will be carried out.

Senator FORD. Well, under superconductor is it your understanding that the Reagan administration budget in their statement said it would be funded from new money?

Dr. HUNTER. I believe that the statement, as I best recall it, sir, and I was not here at the time, was that it would be funded without impacting the funding of the rest of basic science.

Senator FORD. So that means additional funds coming from somewhere?

Dr. HUNTER. Well, I am saying to you, sir, I am not familiar in detail with the statement, as presented, and I have not been privy to the discussions involving the totality of the budget.

Senator FORD. Well, let me ask you then, that as far as you are concerned, OMB has signed off on that particular statement?

Dr. HUNTER. No, sir, I have not been involved in discussions with OMB in which that statement was made to me by OMB or in which I saw any statement of that nature from them. And so I simply can't comment on that authoritatively.

Senator FORD. Dr. Schwitters, in your testimony you indicated that the SSC magnet R&D program has encountered many complex and difficult problems, according to your statement.

Dr. SCHWITTERS. That is correct.

Senator FORD. Could you describe this program for us and describe where you are in magnet development? It is said that DOE—

by DOE that the costs of the SSC are well known because it uses conventional technology. And are you not pushing into totally new areas of technology with your magnet program?

Dr. SCHWITTERS. What we are doing is we are taking a developed technology, superconducting magnets that have been used in the Tevatron at Fermilab, they are being used in a new machine in Germany, they are being worked on in Europe and other places, Japan. We are taking that technology and we are pushing it to its furthest limits to basically reduce our costs.

That program now has been going on for a couple of years. It has been produced—as I indicated, it has produced two fully successful large magnets. These things are big. They are about the width of this room. They, however, as I say, they are pushing the limits of mechanical forces insulation and this sort of thing to find the optimum design.

The most recent magnet in that case turns out did not hold the full current, and we again—my first priority just coming on board here is to take a very hard look so that we understand exactly what is going on in there. But the basic designs have been reviewed now by a lot of people and, as I say, there is a worldwide experience in this area, and so I am confident that the general approach is right. And again, my first priority is go in and see that we are doing all the details just right.

Senator FORD. The last Congress did not authorize construction funding for SSC, but I would say they were fairly liberal with the funding for research and development. What will your strategy be if this Congress does the same thing as it did last Congress?

Dr. SCHWITTERS. Well, as I mentioned before, what is really critical now that we have the performers on board and we have the site is that we get the kind of commitment we need to bring the really first-rate people and industry and so forth on board.

Now, our strategy, of course, is still to mobilize as best we can in Texas and bring in the best people. We will obviously continue while the list that I gave in my oral testimony, which I can provide you a copy of that, but that is our goal right now.

Senator FORD. Dr. Anderson, a couple of quick questions for you and then I will turn it over to my good friend, Senator Bumpers.

In your outline of testimony you stated that you see an imbalance in spending between different fields of science, not only your own. In your own field you see cuts of up to 35 percent in grants. What grants are you talking about and are those DOE grants or NSF grants and just kind of discuss the DOE funding in your field.

Dr. ANDERSON. Yes. The specific grants I was talking about were the NSF grants. I think elsewhere in my testimony I said that the situation in DOE has been relatively more favorable, and I recognize that you gentlemen are, to a great extent, responsible for that.

The situation in NSF is—well, it is a problem which fortunately is not yours and perhaps not appropriate to discuss here. There has not been serious cutbacks, as I understand, in DOE. On the other hand, there has been a number of people who because, of course, we all or many of us juggle different grants from different agencies in many cases, that there has been a hope that DOE could pick up the slack, and sometimes it does and sometimes it does not. Of course, your funding is not increasing at a great rate.

One of the things which is disturbing, and I am not sure that I can—well, again, most of these problems really are not DOE problems. They are these problems in which the majority of computer science is funded by defense money, and much of that money comes through very project oriented stuff and the basic computer scientists are not doing very well.

A very similar thing happens in our own field, particularly with regard to the materials research laboratories where although there is a lot of programmed research and in some institutions like the materials research laboratories in principle, although in practice it does not happen. In principle if you discover something great and it is not according to your deliverables or your thrust, you cannot go ahead and investigate it.

I have not—I do not have good examples. I see the basic energy sciences in particular as one of the better agencies for the funding of basic condensed matter physics. My apprehension is since it has seemed to me that the total research budget of the Department of Energy was pretty sure to stay not roughly constant, that if the SSC were funded, there could well be a cutback in funding for the rest of science.

Senator **FORD**. Senator Bumpers, I am going to turn it over to you now.

Senator **BUMPERS**. Thank you, Mr. Chairman.

I would like to start with asking you, Dr. Anderson. Congressman Barton this morning said one third of our GNP came from technology initiated in high energy physics. Do you consider that an accurate statement?

Dr. **ANDERSON**. No, not at all. I did in my statement say something about that.

I think what the original quote which if I remember rightly came from earlier testimony or a previous year's testimony, by Dr. Triblepeace before this Committee.

I am sure that Dr. Triblepeace, who knows the facts, knows that a very large part of our GNP comes from basic research, but science fragments, grows, changes and what is now called particle physics was in the thirties called nuclear physics and in the twenties did not exist at all.

And the fundamental physics research in the twenties led to some what was called solid-state physics research in the thirties and that research led to some enormous, some very great advantages to economic advantages to our country and to the world.

As I mentioned, there is basic research in genetics which earlier led to such advances as hybridized corn and special kinds of wheat for underdeveloped countries and now is leading to much more sophisticated stuff in molecular biology. But that surely is—does not come of basic research.

But it is perhaps a little ironic that the most important basic piece of basic research that was done that underlies the molecular biology revolution was done in the Cavendish Laboratory during the period when high energy physics was being essentially removed from the Cavendish Laboratory because of a big change in the directorship of the Cavendish Laboratory in Cambridge. So that that happened to be a piece of research that was funded specifically within an organization that had decided to give up the relatively expensive and impractical high energy physics and go into biophysics.

It is a story which certainly does not tell us that particle physics represent a large fraction of our GNP. I do not see how present-day particle physics can even in the long run have very much influence directly on defense or economics.

Of course, I support it as a really fantastic well managed, well run, well thought out intellectual enterprise, and I borrow their techniques, their theoretical techniques all the time. But, on the other hand, they borrow my theoretical techniques, and I do not see that funding or not funding the SSC is going to change that very much.

Senator **Bumpers**. Well, you have pretty much summed up my present feeling about this. I reserve the right to change my mind. As a layman, this all very arcane to me, but I can understand the intellectual curiosity of physicists, high energy particle physicists about this project. But we who sit on this Committee and those of us in Congress as we pointed out time and time again this morning, have to make choices.

I do not mind telling you that is what drives me nuts about the Defense Department. They have never had to make choices, at least in the last eight years. They said we want it all, and they got it all. And we wind up spending $30 billion for an airplane that will not even fly.

Dr. **Schwitters**. Senator, could I comment on your concern there?

Senator **Bumpers**. Certain.

Dr. **Schwitters**. I think the point, as Professor Anderson said, was brought up in earlier testimony about the economic impact. I think the point that Congressman Barton was making and Lea Lederman and others have made is that it is this quest and in these arcane areas initially that have led ultimately to much of the technology around. After all, the work of Maxwell on electromagnetism was a very arcane subject in the 1860s. Now it is the basis of most of our technology.

The work on quantum mechanics, which is a mysterious sounding name in the 1920s was extremely arcane. Today it is the basis for all of the solid-state work that has brought us all the technology of the solid-state work, and what Professor Anderson has commented on.

And so I think the point here is that these long-term, arcane, if you will, aspects of research is what we are talking about. We just do not know what is out there, but we do know.

And what we are doing is we are exploring inner space. We are on a voyage in inner space to understand the basic stuff that we are all made out of and how it works. And in that voyage there are new continents out there. There are discoveries that will be made. You know, we have no idea what they are going to look like and how they will affect us, but they are there and somebody is going to go out and find them.

Senator **Bumpers**. Well, Dr. Schwitters, would you be prepared to make a guess as to what the highest economic or social spin-off—medical, social, however you want to phrase it—spin-off from this project?

Dr. **Schwitters**. I cannot answer that. Again, it is long range. We are looking in the long range here. The immediate spin-offs that we see, again, have to do with our people who are pursuing—they are interested in that arcane science. But as they go along and pursue that, they have other ideas which generate the kinds of spin-offs you are talking about.

I think a good recent example which I would expect to be typical, fairly recent, is the work of Cormack and others where the general idea of these big detectors you see, it was realized that it could be applied to the human body in the sense of the magnetic resonance imaging tools that we now use in many hospitals in this country. That is an area of enormous impact, both in terms of the superconducting wire we use and the basic ideas of detection and so on. That is a huge industry right now which by itself rivals the size of these projects.

Senator **Bumpers**. Have any of your gentlemen visited the Soviet Union in the past few years and had an opportunity to talk to Soviet scientists about high energy physics and particularly the super collider?

Dr. **Schwitters**. I have talked to scientists in this country. I have not visited them in the last few years, though.

Senator **Bumpers**. You have talked to Soviets in this country?

Dr. **Schwitters**. On their visits to this country, yes, sir.

Senator **Bumpers**. Are they putting any significant amount of their resources into this kind of research?

Dr. **Schwitters**. They are building a major new facility that, as was indicated earlier, the UNK machine, which is a large tunnel in their Serpikoff laboratory area which they plan to have a superconducting machine in there in the near future. I am not exactly up to date on the schedule for that.

Senator **Bumpers**. Well, let me ask you this question. And I will address this to you.

What if the Japanese and the Germans and the French and the British and the Italians and whoever else maybe the Saudis came to us and said look, we would like to participate in this and let us all figure out some formula of financial contribution here and let us go ahead and build this $6 billion project. Then the Japanese say we will put up a billion, we say we will put up a billion, the Saudis say well, we will put up a half billion and then the French and so on and the first thing you know, we put it all together. And all they asked for is that their scientists be allowed to participate. And certainly I do not think anybody could object to that. Would you be for that?

Dr. **Schwitters**. That kind of participation is going on more so in this field. It has happened at a machine in Germany where there is the so-called in-kind contribution, and we plan to have that kind of support here. As was indicated earlier, roughly a third or so if you include the Texas contribution.

I personally think the best way to effect that is for this country to take a strong commitment to prove to the world to the rest of these countries that we are going to go ahead with this, and you will see a lot of interest in that.

Senator **Bumpers**. Why do we not wrap that interest up? Why not wrap it up before we undertake the project?

Dr. **Schwitters**. Because it is a classic chicken and egg problem.

Let me make another comment, though. I do see one has to be careful. It is not in this kind of contribution because of the following: The technology transfer aspects of many of these accelerator systems is an important part of this project as they go out through American industries—in fact, international industries.

And the one thing that I am concerned about, I do have a concern in letting that go whole hog, so to speak, and that is that we end up using our industries to dig the dirt, so to speak, while other countries with their in-kind contributions make all the fancy, shiny pieces that we put in the tunnel. And so I think we have to be concerned about the precise levels. It is not so simple that we just want everybody to come on in.

Senator **Bumpers**. Well, Dr. Hunter, tell me what efforts have been made to garner international support for this.

Dr. **Hunter**. Well, sir, we have had informal discussions last year with various parties, and I can supply for the record a summary of those discussions. We have received under the SNT agreement with India a contingent; that is, if we go ahead with the project contribution in-kind of some $50 million, we have a firm letter of support from the Koreans.

I have had personally had discussions with the Japanese in the last few months, in which they have indicated interest and in which they are looking basically for a sign, I think, from the U.S. government that we plan on moving forward with the project.

Dr. Schwitters is leading a group there, within the next month, to explore the details of how they might contribute to the construction of the machine. Let me say that in high energy physics, it is one of the first truly collaborative and international modes of conduct.

There is a custom that big international groups work on detectors. In fact, we have an American scientist, Dr. Ting, who is heading the group in Europe right now, and we expect to see over the next year the formation of these collaborations on the detectors for SSC, which will be a very substantial contribution. But the groups have to form and decide what they want to do precisely.

So, we have asked for and encouraged discussions of those sort on the detector side, and we do expect, for example, in discussions Dr. Schwitters will have with the Japanese, to obtain an understanding of how they might participate and actually fit into the accelerator construction itself.

The contributions are almost inevitably due to their scientists' desires, in the form of in-kind contributions; to provide hardware and people, as opposed to simply providing money and letting someone else do it.

And so, it takes a lot of discussion as to how these things fit together, and, in the case of the SSC, the discussions on the detectors can go forward as the normal customary outflow in the accelerator development area.

In the case of asking for very substantial contributions in other areas, like accelerator construction where you have to go outside the high energy physics concerns in the area, then you need these special discussions, and we are encouraging those at the moment on a detailed level so that the scientists can understand what they can and cannot provide and how it would fit together.

But I do think that the foreign countries are looking strongly for a sign from the U.S. that we really do plan to proceed with the project.

I would comment that one way to handle it is that the Secretary of Energy, in the course of the decision within the Administration, be given the discretion to decide what is an appropriate level for contributions. One way to handle it may be to let the Secretary of Energy basically discuss criteria, once you have decided to go forward or seriously entertain it, as to what level and type of contributions you want to see in accordance with the appropriations each year.

Senator BUMPERS. Who all is participating in the Swiss project?

Dr. SCHWITTERS. There is the LEP project going on now, which is a new machine they are just building in a large tunnel underground.

Senator BUMPERS. How long is that tunnel?

Dr. SCHWITTERS. That tunnel is 27 kilometers around.

Senator BUMPERS. Is the length of the tunnel relevant to different chances of success?

Dr. SCHWITTERS. Yes, but the LEP is a different kind of machine than the Super Collider. It is a so-called electron machine. It actually runs at quite lower energies.

Senator BUMPERS. Let me re-ask the question. Who is participating in that?

Dr. SCHWITTERS. That is the CERN member nations plus Canada; the U.S., as Bob Hunter said——

Senator BUMPERS. What is the estimated cost of that project?

Dr. SCHWITTERS. That project is estimated at about $1 billion Swiss francs, but European estimates do not include salaries as U.S. estimates do. When casted on the same basis as U.S. facilities LEP will cost approximately $1 billion.

Senator BUMPERS. Are we a member of that?

Dr. SCHWITTERS. We are not a member of CERN.

Senator BUMPERS. So we are not going to be participating in that?

Dr. SCHWITTERS. No. Our scientific groups are participating in the experimental program of that machine, but that machine runs at a considerably lower energy than what we are talking about with the Super Collider.

Let me just clarify the point. What has been talked about a potential future machine, (LHC), that would displace the current machine that they are just building and completing there, and be the kind of collider we are talking about. That is a future thing, and again where you take the existing facilities, displace the equipment in there, put in a new device in the existing tunnel, and that device is the one that has been considered as a possible alternative to the SSC.

That device would be roughly something under a half of our energy. The one that is going in now is considerably different.

Senator BUMPERS. How long will it take to build this thing, Dr. Hunter?

Dr. HUNTER. The SSC, sir?

Senator BUMPERS. Yes.

Dr. HUNTER. Approximately eight years.

Senator BUMPERS. What would you estimate in today's dollars would be the annual cost for operating it?

Dr. HUNTER. When you were out of the room, sir, Senator Ford had a number, which was roughly $300 million in FY 1988 dollars for the operations cost.

Senator BUMPERS. How has the scientific community, particularly the physicists—has the American Physical Society taken a position on this?

Dr. HUNTER. Sir, let me just comment there. To the best of my knowledge, they have not taken an official position. The immediate past head, Dr. Fitch, I believe strongly supports the program. I do know that roughly two to three years ago Dr. Sidney Drell, who was the president of the society, strongly supported the project. Basically, the physicists within the high-energy physics area very strongly support this project and have just this month reiterated that support, as this is the machine that is needed to ensure that the program in high-energy physics can move forward and maintain a leadership quality.

Senator BUMPERS. What would the scientific community think, how do you think they would feel if we were not going to put any new money in and just simply take money from other scientific or basic research projects in this country and put it into this? How would they feel about it?

Dr. HUNTER. Different parts of them would feel differently, sir, I am certain, depending on where you plan to take it or how you would do it. But we have exerted every effort to ensure that that does not happen. My office funds a lot of the supporting work that goes on, for example, in condensed matter physics, and those budgets have risen.

We are building several new facilities, in fact, accelerator-based facilities. The accelerator technology of some 10 to 20 years ago is used to support condensed matter physics and surface physics, and chemistry, so we do not view the SSC as a unique instrument in that it is a large instrument. There are many large instruments we are building today in many areas, and we intend to see that we maintain our leadership in basic research in those areas that we are involved in.

We do not want to have a second-class program. The SSC is the tool we need to maintain that leadership in the high-energy physics area, and that is how we arrived at the proposal before you. But it is not a uniquely large instrument in the sense that we build many large instruments that are used by many users, as the SSC would be.

Senator BUMPERS. You mentioned a mentioned a moment ago the Soviet Union apparently indicated to me that they were going on a crash basis on high-energy particle physics to build a similar project. Did I misunderstand you?

Dr. HUNTER. Well, sir, I believe that Dr. Schwitters made the comment about the Soviet effort. I do not believe that I made a comment that the Soviet Union is going on a crash basis.

Senator BUMPERS. Do you have any idea how big theirs is going to be?

Dr. **Schwitters**. Well again, they are building this machine which is 3 TeV—trillion electron volts.

Senator **Bumpers**. That is smaller than the CERN facility, is it not?

Dr. **Schwitters**. That is correct, but the Soviet UNK facility is under construction now.

Senator **Bumpers**. Well, the CERN facility is going to cost less than a half billion and we are talking about $6 billion here, and the Soviets' is going to be smaller than the CERN.

Dr. **Schwitters**. The budget figure I gave you is for the LEP machine they are building today. That is a different machine than the one that is considered a competitor to the Super Collider.

Senator **Bumpers**. I do not follow you. Say that again.

Dr. **Schwitters**. CERN is building a machine today in this large tunnel near Geneva, that is a so-called electron-positron collider. It runs at an energy of about a few percent of the energy of the Super Collider. That is the machine that had the official budget of roughly a billion Swiss francs. That is a machine going on today. What is considered, or what is discussed in various quarters as a potential future alternative to SSC is to use that tunnel, displace the current activity that they are just about to finish next summer and build a different machine, a collider like the Super Collider. That is not yet a program at CERN, it is just an idea.

Senator **Bumpers**. Dr. Hunter, how much in your budget for research into superconductivity?

Dr. **Hunter**. Sir, in the energy research budget there is approximately $22 million this year. In the departmental budget I believe there is approximately $43 million for research into superconductivity and the phenomena of it.

In other areas, such as the magnetic fusion area, we have research on superconducting techonology-making coils, and certainly, for example if you included the research on the magnets here, there would be a great deal more. But those are the numbers associated with basic research on high temperature superconductivity.

Senator **Bumpers**. When it comes to fusion research, could we speed up the possible success of accomplishing fusion energy in this country by putting a lot more resources into it, or are we doing all we can do?

Dr. **Hunter**. Sir, one of the first things that I did after coming into this office six to seven months ago was to undertake an evaluation of where we stand on the key physics part of fusion, called confinement physics. In other words, how well the magnetic fields hold the energy in so the stuff can eventually fuse.

We are part-way through that, and we have a committee set up. We have spent a lot of attention and do not have an answer yet, but, as I told the House Science, Space, and Technology Committee a few days ago, we will be putting out some policy guidance soon. As a result of our conclusions, we have looked at that question.

The key issue is that there is some physics we simply do not understand in these machines. We have to understand that as quickly as we can to be able to definitively answer the question of confinement physics, but we take it very seriously. As an energy alternative fusion has certain environmental advantages. On the other hand, it has been with us for some time, and it has not yet produced net energy.

So we are examining that closely and will be happy to give you the outcome of this study as soon as it is available, sir.

Senator **Bumpers**. I hope you will, because it is an item of great interest to me. Let me ask any one of you or all of you, is it possible to use particle accelerators to produce tritium?

Dr. **Hunter**. Yes, sir. There have been some proposals made, in fact some very early proposals in the 1950s, as a way to make tritium, as opposed to using reactors. The country chose back then to build reactors, which have been a very steady and reliable source of tritium, but there are alternatives.

They involve the use of proton accelerators, typically, at lower energies than the ones we are talking about here, to bombard materials and produce neutrons that then generate tritium. There are those possibilities. Yes sir.

Senator **Bumpers**. How do you evaluate them?

Dr. **Hunter**. To date, we think that it is very early for them. The numbers in the actual existing accelerators indicate, that those energies lead to efficiencies that are not extremely attractive, so you are talking about having to do technology development and, also, you are talking quite a bit of complexity in the target area.

So it is my personal opinion that, we view them as very interesting, but we do not view them as being anywhere near technologically sufficiently advanced to become a main-line alternative solution to the near-term problems.

Senator **Bumpers**. Mr. Chairman, I think that concludes my questions.

Senator **Ford**. Senator Heflin, do you have some questions?

Senator **Heflin**. I have a statement that I would like to be entered into the record.

Senator **Ford**. Your statement will be included in the record as if given.

[The prepared statement of Senator Heflin follows:]

Statement of Hon. Howell T. Heflin, U.S. Senator From Alabama

Mr. Chairman, today, this Senate Energy Subcommittee on Research and Development is conducting hearings on one of the most important aspects of DOE's fiscal year 1990 budget requests—Energy Science. If the United States is to remain the world leader in scientific technology and advancement, then, it must continue to produce the necessary resources—brain power and finances.

I have always been a very strong advocate of energy research and development. However, as with everything else in life, we must strike a meaningful balance between what was, is, and will be.

The Department of Energy is asking for $1.2 billion for generai science programs in fiscal year 1990. It is my understanding that this figure represents almost a quarter billion dollars ($2.47 million) increase over the current fiscal year's funding.

Included in this amount is DOE's $250 million request for the superconducting super collider [SSC], a proposed particle accelerator intended to expand knowledge of the fundamental nature of matter and energy. For this fiscal year, Congress appropriated $98.6 million for SSC research and development, but refused to authorize construction. For fiscal year 1990, the administration would allocate $160 million of the $250 million for the SSC facility construction.

Mr. Chairman, I have always wholeheartedly supported the SSC. But, in good conscience, and in the interest of all DOE science programs, I believe that this subcommittee must examine very closely the impact that SSC funding will have on other DOE science programs. I would like for this subcommittee to hear from the scientific community, as a whole, with regard to its level of support for SSC if certain budget restrictions are going to cause significant funding reductions in the other DOE science programs.

Finally, Mr. Chairman, although I continue to support the SSC, I am gravely concerned about the October 1988 report of the CBO entitled, "Risks and Benefits of Building the Superconducting Super Collider". As you well know, in that report, the CBO opposes construction of the SSC at this time. In my judgment, I think that we should, at least, analyze in detail this report and weigh the risks versus the benefits of constructing the SSC at this time. I believe that the poor fiscal health of our national economy today compels us to act very prudently on this proposed multi-billion dollar project.

Thank you.

Senator **Heflin**. I do not have any questions.

Senator **Ford**. Thank you very much. Dr. Hunter, I believe you have some correspondence from CBO about risk and benefits of building the SSC. Could we have those for the subcommittee files?

Dr. **Hunter**. Yes, we will provide those to you.

Senator **Ford**. We thank you, gentlemen, and appreciate your patience and interest, and we may be submitting some questions to you at a later date for answers in writing rather than more testimony, and we would appreciate it if you would do that, because I know Dr. Anderson has been very, very helpful with statements and answers to questions for us.

So this hearing is recessed.

[Whereupon, at 12:10 p.m., the hearing was adjourned.]

APPENDIXES

APPENDIX I

Responses to Additional Questions

Department of Energy
Washington, DC 20585

March 16, 1989

The Honorable Wendell H. Ford
Chairman
Subcommittee on Energy Research
and Development
Committee on Energy and Natural Resources
United States Senate
Washington, D.C. 20510

Dear Mr. Chairman:

On February 24, 1989, Dr. Robert O. Hunter, Jr., Director Office of Energy Research appeared before your subcommittee to discuss the Department's Fiscal Year 1990 budget authorization for energy research programs and the Superconducting Super Collider.

Following that hearing you submitted written questions, including questions on behalf of Senator McConnell, for our response to supplement the record. Enclosed are the answers to those questions, which also have been sent to Senator McConnell.

If you have any questions, please have your staff call Frances Bryant on 586-4277. She will be happy to assist.

Sincerely,

Robert G. Rabben
Assistant General Counsel
for Legislation

Enclosures

cc: The Honorable Pete V. Domenici
Ranking Minority Member

POST-HEARING QUESTIONS AND ANSWERS
FROM THE
FEBRUARY 24, 1989, HEARING
BEFORE THE
SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT
COMMITTEE ON ENERGY AND NATURAL RESOURCES
U.S. SENATE

WITNESS: DR. ROBERT O. HUNTER, JR.

DIRECTOR, OFFICE OF ENERGY RESEARCH

QUESTIONS FROM SENATOR WENDELL H. FORD

Congressionally Initiated Projects

Question 1: In the budget book describing your request for Biological and Environmental Research (at page 324) you indicate that no provision is made in the BER budget for $20 million of Congressionally-initiated projects.

In the description of Basic Energy Sciences (at page 411) you indicate that $39.6 million in Congressionally-initiated projects are not funded in the BES budget.

What are the projects we are talking about here?

Why aren't they being funded?

Please provide the Subcommittee with a list of the Congressionally-initiated projects that you are proposing not to fund. Explain what each project is about, who proposed it, and tell us why you are not funding it.

In addition, include a list of the comparable projects that are proposed to be funded in each of these two budgets and the rationale for these projects.

Answer: A number of "bricks and mortar" construction projects have been added to the budget request by the Congress, in each of the last several years. The Department and the Congress have on various occasions debated the issue of continued funding for these types of projects in light of the tight budgets provided for research. This Administration is committed to submit to the Congress only those projects with a direct programmatic interest to the Department. The projects you are referring to, which were added by Congress, are not essential to carrying out the Department of Energy's programmatic mission. As a result, as in past years, the FY 1990 budget request does not include funding for any of the Congressionally initiated "bricks and mortar" construction projects.

It is our understanding that Congress intended to fully fund in FY 1989 those projects included in the FY 1989 appropriations act, which were proposed to the Congress by the respective benefiting institutions. I will provide as an insert for the record a short description of the FY 1989 Congressionally mandated projects.

The DOE has not requested in its FY 1990 budget any comparable building ("bricks and mortar") projects. The projects for which DOE has requested funding essentially represent advanced scientific instruments or tools associated with the scientific research support needed to carry out the programmatic missions of the Department.

COMMITTEE: SENATE ENERGY & NATURAL RESOURCES
SUBCOMMITTEE: ENERGY RESEARCH & DEVELOPMENT
DATE: 2/24/89
WITNESS: DR. HUNTER
PAGE: 65
LINE: 4

INSERT FOR THE RECORD

FY 1989 CONGRESSIONALLY FUNDED PROJECTS INCLUDED IN THE BIOLOGICAL AND ENVIRONMENTAL RESEARCH PROGRAM

Basic Science Building (The Oregon Health Sciences University, Portland, Oregon)

The Basic Science Building will consolidate the basic science departments of the School of Medicine, expand basic research space, upgrade educational space, provide additional dining space and improve site circulation. The facility will consist of approximately 130,000 gross square feet of space for laboratories, offices, teaching facilities and related support services.

Cancer Research Center (Medical University of South Carolina)

The Cancer Research Center is planned to provide state-of-the-art cancer treatment options; develop, through research, new cancer control strategies; and educate future generations of cancer specialists and scientists. The Center is planned to be a multi-story facility providing approximately 75,000 gross square feet of space to house research laboratories, patient treatment areas, and administrative and related support functions.

FY 1989 CONGRESSIONALLY FUNDED PROJECTS INCLUDED IN THE BASIC ENERGY SCIENCES PROGRAM

Center for Science and Engineering Technologies (Arizona State University)

The Center for Science and Engineering Technologies will provide facilities, equipment and services for interdisciplinary research in physical and life sciences with direct applications to new technology; innovations in high technology fields such as manufacturing techniques, computers, semi-conductors, telecommunications, airframes and artificial intelligence; and the accelerated development of graduate physicists, chemists, mathematicians, material scientists and computer scientists.

The proposed facility is a new multi-story building of approximately 101,500 gross square feet located in the midst of the existing science and engineering complex. The facility will provide research, education and administrative space.

The Center for Automation Technology (Drexel University)

The Center will be a state-of-the-art facility engaged in the conduct of basic engineering research on advanced concepts and systems, graduate and undergraduate education in the principles of automation and information technologies, and assistance in translating basic engineering research ideas for automation devices and systems into products for the market place. The new facility will be a multi-story building of approximately 54,000 gross square feet. It will house flexible laboratories, teaching space, and faculty/administrative offices and their support functions.

Center for Advanced Microstructures and Devices (Louisiana State University)

The purpose of the proposed Center for Advanced Microstructures and Devices is to conduct materials research bearing on advanced microstructures and devices, and to educate young scientists and engineers in the use of advanced materials research instrumentation.

The facility will include an electron storage ring and other materials science instrumentation, and approximately 31,000 square feet of building space for laboratories, experimental space, office and seminar rooms.

Proton-Beam Demonstration Cancer Treatment Center (Loma Linda University Medical Center)

The facility is a proton treatment center that will utilize a dedicated variable-energy proton synchrotron in a clinical setting for treatment of cancer and other diseases. The Center is designed primarily as an outpatient facility that will serve the Loma Linda University Medical Center and other local hospitals. The facility will be a regional and national resource for radiation therapy that will provide educational and research facilities for planning and development of other facilities.

The planned facility will include approximately 126,000 square feet containing space for patient examination and treatment, staff offices, laboratories, treatment planning computer work stations, and administrative and related support space. The facility will include a 70 to 250 MeV proton synchrotron, beam transport, beam lines, and safety and control systems.

QUESTIONS FROM SENATOR WENDELL H. FORD

Global Warming

Question 2:	In your budget book discussing the research you are proposing on carbon dioxide and the greenhouse effect (at page 321) you say that "The current science base for the carbon dioxide issue is not ready for energy policy action." That seems to mean that you don't think we ought to be making major changes in our energy policy to deal with carbon dioxide production. What do you mean by that sentence? What research are you carrying out to determine what energy policy actions are required by the evidence from global warming?
Answer:	While DOE believes that the scientific connection between global warming and increased carbon dioxide and other trace gases has not yet been quantitatively verified, there are a number of actions which should be undertaken by both government and the private sector which are justified without reference to global warming, but which would reduce carbon dioxide emissions. These include investment in energy efficiency improvements and the further development of nonfossil energy sources such as renewables and nuclear. The state of the science does not yet support projections of global warming with sufficient confidence to justify serious dislocations in energy generation and use. Although a scientific assessment justifying mitigative responses is not yet available, it is advisable to begin evaluating both the possible effects of global warming and potential mitigative responses for cost and effectiveness.

The Office Energy Research is engaged, together with other agencies, in a multidisciplinary research program aimed primarily at understanding the climate's response to the rise of the greenhouse gases. This research is designed to provide a sound foundation for future energy policy actions.

QUESTIONS FROM SENATOR WENDELL H. FORD

Congressional Budget Office Report

Question 3: In October, 1988, the Congressional Budget Office published a report entitled "Risks and Benefits of Building the Superconducting Super Collider" that seems to suggest that the United States should join with the Europeans in a cooperative project in Europe rather than build the SSC at much higher cost.

What is your reaction to this suggestion by CBO?

Answer: The Large Hadron Collider (LHC), referred to in the CBO report, is not a viable substitute for the SSC. Furthermore, the project has not yet been approved by CERN. The design work and magnet R&D are roughly at the point the Superconducting Super Collider (SSC) work was at several years ago. In reaching our decision to build the SSC, we explored many possible alternatives, including the LHC, but concluded that no viable options exist. In particular, the LHC, at 40 percent of the energy of the SSC, would not be nearly as scientifically capable as the SSC. The state of the science demands a collider at the energy of the SSC, and we concluded that only the U.S. is in a position to undertake such a technological challenge.

Moreover, the LHC faces difficult technical challenges that will require considerably more R&D than has been done, in particular, in superconducting magnets and detectors. The LHC does not have a detailed conceptual design, definitive cost estimates, or the financial backing of the CERN member governments.

Serious logistical difficulties also exist: The LHC is to be built in the same tunnel as the existing Large Electron-Positron (LEP) collider, which is to start operation this summer. There will be strong pressure from the scientists using LEP to realize fully the scientific potential of LEP. This demand for more LEP running time would interfere with full utilization of the the LHC and vice versa. Furthermore, fewer interaction regions would be available at the LHC than at the SSC. Consequently, the number of experiment-hours of operation on LHC would be about one quarter of that of the SSC, only a fraction of which would be available to U.S. scientists. This would not meet U.S. research needs.

If the U.S. were to join with CERN in the LHC in lieu of the SSC, all of the world's leading high energy physics research would be in a single tunnel in Switzerland. Most people believe this would not be prudent.

Without the SSC, the U.S. would lose the advantages that accrue from building the accelerator on its own soil and doing physics at home rather than abroad. The national pride and prestige and encouragement to young people to enter scientific fields that come from pursuing the frontier in this fundamental field would be lost. Training opportunities for U.S. scientists, engineers, and technicians in many related advanced technologies would also be foregone. The continued stimulation of U.S. industry in many high technology areas would no longer be domestically available and some of our best scientists would go abroad to pursue their interests in fundamental science.

For these reasons, we consider international collaboration with the SSC as more promising, far wiser, and of more benefit to the Nation than the LHC or any other alternatives.

QUESTIONS FROM SENATOR WENDELL H. FORD

Non-Federal Contributions to SSC

Question 4: Your budget request for SSC estimates the cost of the project at $5.9 billion. You assume that non-Federal entities are going to contribute $1.8 billion, so that the net cost to the federal government will be about $4.1 billion.

What are these non-federal sources, and how many of them have pledged contributions to the project?

Please provide the Committee with a listing of these sources and how much they will contribute. Distinguish cash and in-kind contributions.

Answer: The non-Federal sources include the State of Texas and potential foreign contributors.

Although the State of Texas has pledged a total of approximately $1 billion, we have not yet had definitive discussions with them to determine precisely what their contribution will include. Greater detail on the Texas contribution is provided below, in our response to question 8.

With regard to foreign participation and contributions, DOE held formal government-to-government discussions with the six Economic Summit countries (England, France, Italy, West Germany, Canada, and Japan) in January 1988. Officials of all of these governments said they were interested in continuing discussions on the SSC. It is expected that scientists from most of these countries will participate in building the detectors for and carrying out experiments on the SSC. But only a few of these countries (Japan, Canada, and Italy) may be interested in helping to construct the accelerator.

During the last year, informal discussions have been held with Switzerland, Spain, and Korea. All of these countries, too, are interested in participating. South Korea has sent a letter confirming its interest, and it is likely that Spain and Switzerland will send similar letters. In the extension of the India Science and Technology Agreement, India has agreed to furnish up to $50 million of in-kind contributions.

It is simply too early to give a list of which countries may contribute to the construction of the SSC accelerator and what their contributions will be. Most countries are waiting until Congress acts to approve construction of the SSC before committing anything.

We also have the uncertainty of what restrictions might be placed on international contributions by legislation such as those in H.R. 4505, the Department of Energy Civilian Energy Research and Development Authorization Act of 1989, introduced in the last Congress. Such restrictions on contributions from abroad could make it impossible to reach the $1.8 billion goal. Even without limitations, the goal may be difficult to reach.

QUESTIONS FROM SENATOR WENDELL H. FORD

SSC Magnets

Question 5: I have heard it estimated that the SSC project has benefited from about $200 million in appropriations to develop the magnets that are so crucial to operation of the SSC.

Is this accurate? If not, what is the right figure?

How many of the magnets built so far have performed adequately?

How many have failed?

What is the reason for the failures?

Is the SSC magnet program behind schedule? By how much?

Answer: The total funding for SSC R&D over 6 fiscal years, 1984-1989, is $212 million. Of this, about 60 percent has been spent on magnets. I have a funding history which I would like to submit for the record.

To date, over thirty short magnets and twelve full-size magnets have been built. The short magnets have an excellent performance record. Of the ten full-size magnets tested to date, all have been productive in a R&D sense, contributing to our understanding of the magnets. The early full-size magnets met many of the requirements, but had difficulty in achieving full field. The field problem in the early magnets was traced to mechanical clamping of the superconducting coil and has been fixed. Two recent magnets have been fully successful in this regard. The most recent magnet behaved very stably, but fell short of the desired field level. This is believed to be due to quality control problems in cable manufacture or handling and not to a magnet design problem.

Two magnets were tested to failure and developed electrical short circuits to ground. This led to the formation of arcs which melted some metal and burned a hole in the helium vessel.

While a lesser number of magnets have been tested than earlier had been planned, the SSC magnet program is on schedule to support the planned start of large-scale production late in 1992 and full production in 1993.

SSC Funding
(B/A in Millions)

	FY 1984	FY 1985	FY 1986	FY 1987	FY 1988	FY 1989	FY 1990 Request
Operating	$ 20.0	$ 20.0	$ 20.0	$ 20.0	$ 33.0	$ 82.6*	$ 69.0
Equipment	-----	-----	-----	-----	-----	16.0	21.0
Construction	-----	-----	-----	-----	-----	-----	160.0
Total SSC	$ 20.0	$ 20.0	$ 20.0	$ 20.0	$ 33.0	$ 98.6	$ 250.0

---

* Excludes $1.387M, the SSC portion of the $12M general reduction contained in FY 1989 Appropriation.

QUESTIONS FROM SENATOR WENDELL H. FORD

New Technology and the SSC

Question 6: It is argued that the cost estimates for the SSC are accurate because the project will use existing technology. It is also argued that there will be technological spinoffs from the project. But this latter contention seems to imply that new technology will be developed by the SSC.

Which is it? Conventional technology or new technology?

Answer: The SSC accelerators and storage rings are based on well-established, conservative technology of the type that is used at other facilities in the U.S. and at other world-class high energy physics research facilities. However, the SSC is stretching these technologies to new performance limits in order to get the best performance at lowest cost. Technological benefits to other fields and to industry will result from such extensions of established technologies.

R&D for the SSC has already improved the capability/performance of superconducting cable by a factor of one and a half. This will benefit other applications such as Magnetic Resonance Imaging (MRI), now used in many hospitals for noninvasive diagnostics. The high interaction rates of the SSC are leading to improvements in detector technologies and in high speed electronics. Improved computerized-beam diagnostics and beam control systems can also be expected to have further applications. R&D experiments on nonlinear effects on particle beams, motivated by the SSC and carried out at Fermilab, have already led to new results of interest to other fields of research as well as to the design and operation of other accelerators.

QUESTIONS FROM SENATOR WENDELL H. FORD

SSC and the High-Energy Physics Budget

Question 7: Please provide the Subcommittee with a projection of your high-energy physics budget request for the period of construction of the SSC showing which components of high-energy physics funding will increase and which will decrease.

Answer: Funding projections for the High Energy Physics program for the fiscal years beyond FY 1994 have not yet been established. However, the following is a table that shows the budget projections for the period FY 1990 thru FY 1994.

HIGH ENERGY PHYSICS
Outyear Projections
(dollars in thousands)

	FY 1990	FY 1991	FY 1992	FY 1993	FY 1994
Total, Budget Authority	$616,230	$684,900	$728,870	$766,370	$824,370

As the SSC nears completion and initial operation, we would expect to decrease other non-SSC components of the High Energy Physics program. The exact pattern of these changes must be determined by the physics priorities of that time. The overall physics priorities in the late 1990's will depend on the results of key experiments, many of which have not yet been planned. For example, the Stanford Linear Collider at SLAC is just now being turned on for physics and the full impact of the results from this run will not be known for a few years.

QUESTIONS FROM SENATOR WENDELL H. FORD

Texas' Contribution

Question 8: Provide the Subcommittee with an analysis of the $1.1 billion that Sen. Gramm says the State of Texas is going to "put on the table".

Is this cash?

Does this figure include the value of lands provided?

Does this figure include in-kind contributions? If so, please identify them.

Answer: The Texas National Research Laboratory Commission (TNRLC) has been authorized by the State of Texas to transfer all land required for the SSC at no cost to the Federal government and to provide more than $1 billion to support construction and operation of the SSC. The latter does not include the cost of land, which will be covered separately by the State. It is our understanding that TNRLC is flexible in the consideration of how best to use these funds for construction and operation of the SSC. Suggestions by Texas for the application of these funds include: 1) as much as $700 million to help defray the costs to construct the civil works required for the SSC; 2) as much as $175 million to lower the cost of electricity; 3) external research program support of as much as $100 million; 4) creation of a Center for Technology Development and Transfer; and 5) personnel support services. The exact use of the Texas funds will be discussed by the Department and TNRLC and an agreement will be reached to use these funds in the optimal way for the project.

QUESTIONS FROM SENATOR WENDELL H. FORD

Accelerators and Tritium

Question 9: As you know, one of the most difficult problems facing the next Secretary of Energy is the prospect that the reactors at Savannah River and Hanford may not be restarted on a timely schedule to produce the tritium needed by the U. S. nuclear weapons program. It has been suggested that particle accelerators could be used to produce tritium for the U. S. nuclear weapons program.

Please provide the Subcommittee with a report evaluating the potential for use of one or more DOE accelerators for tritium production.

Is this feasible? How would it work?

What do we know about costs?

What existing facilities could be used?

Answer: Electroproduction of tritium is the latest version of a historically long list of proposals to produce fissile or other types of speciality radioactive material using accelerators. The original concept was proposed for accelerated protons and a prototype low frequency linear accelerator studied by E. O. Lawrence, C. Van Atta and others of the Lawrence Livermore National Laboratory in the late 40's. The idea surfaced again in the early seventies, was reviewed by the Energy Research and Development Administration (ERDA) at that time, and was rejected as not being a cost effective alternative to breeder reactors for producing fissile material.

The present proposal to use very high intensity radiofrequency lines to electroproduce tritium by bombardment of lithium with an intense beam of protons has recently been put forward by a number of national laboratories and industrial firms. These proposals were reviewed by the Department of Energy (DOE) and by the JASON Group (on their own initiative) over the last year. The conclusions are that, as a technical alternative, the electroproduction of tritium looks interesting. The DOE has decided, however, that since the accelerator production of tritium is unproven and tritium production is a very serious concern, it is better to proceed with proven technology. Consequently, DOE is proposing to build new reactors. The JASON Group is more enthusiastic, commenting that the electroproduction of tritium "deserves the most serious consideration as an alternative."

The technical basis of the proposals for Accelerator Production of Tritium, or APT, is a proton linear accelerator operating in a range of from 1 to 1.6 GeV (Gigaelectron Volts) and 250 to 1000 milliamperes of current. While there is extensive experience since the late 1940's with proton linacs, no machine approaching these performance parameters exists. Proposals presented indicate that from 5 to 7 years would be required to design and construct a facility. This does not include preconstruction R&D, an essential item, which is estimated to take at least four years, nor an estimate of the real difficulties of building and operating the target facility, a technical problem which appears to be quite challenging and about which there is little known, outside of paper studies.

All of the studies done for the proposals are of a preliminary nature and probably underestimate the R&D required, the time to carry out the R&D, the total facility cost to build and operate, and the facility reliability.

As a longer term alternative to reactor production of tritium, the APT may be every bit as attractive as the JASONS believe it is, but this technology cannot be taken as a serious alternative to existing tritium production facilities, in the absence of actual demonstrations of the feasibility of major subsystems. The only existing facility of value in doing such tests at present is the Los Alamos National Laboratory Meson Physics (LAMPF) accelerator which operates at an energy of 800 MeV (0.8 GeV) and a current approaching 1 milliampere. This is about two thirds of the projected design energy required for tritium production and about a factor of 1000 less than what many specialists believe is the operating current required for an adequate tritium production rate. Consequently, it would be necessary to build a new high current facility in order to test the design concepts of the APT.

J. BENNETT JOHNSTON, LOUISIANA, CHAIRMAN

DALE BUMPERS, ARKANSAS
WENDELL H. FORD, KENTUCKY
HOWARD M. METZENBAUM, OHIO
BILL BRADLEY, NEW JERSEY
JEFF BINGAMAN, NEW MEXICO
TIMOTHY E. WIRTH, COLORADO
KENT CONRAD, NORTH DAKOTA
HOWELL T. HEFLIN, ALABAMA
JOHN D. ROCKEFELLER IV, WEST VIRGINIA

JAMES A. McCLURE, IDAHO
MARK O. HATFIELD, OREGON
PETE V. DOMENICI, NEW MEXICO
MALCOLM WALLOP, WYOMING
FRANK H. MURKOWSKI, ALASKA
DON NICKLES, OKLAHOMA
CONRAD BURNS, MONTANA
JAKE GARN, UTAH
MITCH McCONNELL, KENTUCKY

DARYL OWEN, STAFF DIRECTOR
D. MICHAEL HARVEY, CHIEF COUNSEL
FRANK M. CUSHING, STAFF DIRECTOR FOR THE MINORITY
GARY G. ELLSWORTH, CHIEF COUNSEL FOR THE MINORITY

United States Senate
COMMITTEE ON
ENERGY AND NATURAL RESOURCES
WASHINGTON, DC 20510-6150

February 27, 1989

Honorable Robert Hunter
Assistant Secretary
Office of Energy Research
U. S. Department of Energy
Washington, D.C. 20585

Dear Mr. Hunter:

I want to take this opportunity to thank you for providing testimony on the Department of Energy FY90 budget request for energy research programs in particular the Superconducting Super Collider. I feel the testimony received during the hearing was quite valuable and your comments will be very beneficial as we move closer to consideration of this budgetary issue.

As a follow-up to the hearing I have enclosed questions to be answered and included in the printed hearing record. I would appreciate receiving the answers to these questions by close of business March 13, 1989. If you have any questions concerning this request please contact Ben Cooper or Teri Curtin at 224-7569.

Thank you for your assistance in this matter.

Sincerely,

Wendell Ford
Wendell H. Ford
Chairman, Subcommittee on Energy
Research and Development

FOLLOW-UP QUESTIONS FOR DR. ROBERT HUNTER
from
SENATOR WENDELL H. FORD

Hearing on Energy Research and the SSC
February 24, 1989

1. Congressionally-initiated projects

In the budget book describing your request for Biological and Environmental Research (at page 324) you indicate that no provision is made in the BER budget for $20 million of Congressionally-initiated projects.

In the description of Basic Energy Sciences (at page 411) you indicate that $39.6 million in Congressionally-initiated projects are not funded in the BES budget.

What are the projects we are talking about here?

Why aren't they being funded?

Please provide the Subcommittee with a list of the Congressionally-initiated projects that you are proposing not to fund. Explain what each project is about, who proposed it, and tell us why you are not funding it.

In addition, include a list of the comparable projects that are proposed to be funded in each of these two budgets and the rationale for these projects.

2. Global Warming Research

In your budget book discussing the research you are proposing on carbon dioxide and the greenhouse effect (at page 321) you say that -

"The current science base for the carbon dioxide issue is not ready for energy policy action."

That seems to mean that you don't think we ought to be making major changes in our energy policy to deal with carbon dioxide production.

What do you mean by that sentence?

What research are you carrying out to determine what energy policy actions are required by the evidence from global warming?

3. Congressional Budget Office Report

In October, 1988, the Congressional Budget Office published a report entitled "Risks and Benefits of Building the Superconducting Super Collider" that seems to suggest the the United States should join with the Europeans in a cooperative project in Europe rather than build the SSC at much higher cost.

What is your reaction to this suggestion by CBO?

4. Non-Federal Contributions to SSC

Your budget request for SSC estimates the cost of the project at $5.9 billion. You assume that non-federal entities are going to contribute $1.8 billion, so that the net cost to the federal government will be about $4.1 billion.

What are these non-federal sources, and how many of them have pledged contributions to the project?

Please provide the Committee with a listing of these sources and how much they will contibute. Distinguish cash and in-kind contributions.

5. SSC Magnets

I have heard it estimated that the SSC project has benefitted from about $200 million in appropriations to develop the magnets that are so crucial to operation of the SSC.

Is this accurate? If not, what is the right figure?

How many of the magnets built so far have performed adequately?

How many have failed?

What is the reason for the failures?

Is the SSC magnet program behind schedule? By how much?

6. New Technology and the SSC

It is argued that the cost estimates for the SSC are accurate because the project will use existing technology. It is also argued that there will be technological spinoffs from the project. But this latter contention seems to imply that new technology will be developed by the SSC.

Which is it? Conventional technology or new technology?

7. SSC and the high-energy physics budget

Please provide the Subcommittee with a projection of your high-energy physics budget request for the period of construction of the SSC showing which components of high-energy physics funding will increase and which will decrease.

8. Texas' Contribution

Provide the Subcommittee with an analysis of the $1.1 billion that Sen. Gramm says the State of Texas is going to "put of the table".

Is this cash?

Does this figure include the value of lands provided?

Does this figure include in-kind contributions? If so, please identify them.

9. Accelerators and Tritium

As you know, one of the most difficult problems facing the next Secretary of Energy is the prospect that the reactors at Savannah River and Hanford may not be restarted on a timely schedule to produce the tritium needed by the U.S. nuclear weapons program. It has been suggested that particle accelerators could be used to produce tritium for the U.S. nuclear weapons program.

Please provide the Subcommittee with a report evaluating the potential for use of one or more DOE accelerators for tritium production.

Is this feasible? How would it work?

What do we know about costs?

What existing facilities could be used?

SENATOR MITCH MCCONNELL'S QUESTIONS FOR
DR. ROBERT HUNTER
DIRECTOR, OFFICE OF ENERGY RESEARCH
SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT HEARING
FEBRUARY 24, 1989

It is my understanding your office operates a combustion research facility at the Sandia Lab in Livermore, CA.

I understand the facility is used by commercial oil and coal companies and officials from DoE's Fossil Energy and Energy Conservation departments to conduct studies using lasers to analyze the chemistry of the combustion of various fossil fuels.

FYI 1990 funding request for the facility is $4.5 million, up from $3.5 million in FY 1989.

--Can you give us a sense of the magnitude of research being done at the Sandia facility?

--What portion of the research involves coal combustion?

--Do you ~~have~~ know what portion of the funds requested FY 1990 are targeted for coal combustion research?

--Are there plans to increase spending on coal combustion research?

Department of Energy
Washington, DC 20585

February 24, 1989

Dr. Ben Cooper
Committee on Senate Energy
and Natural Resources
312 Hart Senate Office Building
Washington, DC 20510

Dear Ben,

Enclosed you'll find correspondence that DOE would like to submit for the record to the February 24, 1989 authorization hearing on Energy Research.

Sincerely,

Jeffrey W. Jepsen

Jeffrey W. Jepsen

Enclosure

Department of Energy
Washington, DC 20585

NOV 9 1988

Mr. James Blum
Acting Director
Congressional Budget Office
House of Representatives
Washington, DC 20515

Dear Mr. Blum:

I am writing to you to summarize the Department of Energy's views of the recent Congressional Budget Office (CBO) report, "Risks and Benefits of Building the Superconducting Super Collider."

During preparation of this report, Department personnel, along with academic scientists associated with the Superconducting Super Collider (SSC) project, met with the principal author, Mr. Philip Webre, and were asked to comment informally on the August 1 and September 28 drafts of the report. I believe our discussions were very helpful, and we appreciate the opportunity to offer suggestions.

The Department appreciates the strong interest and great efforts evidently displayed by your staff in looking into the more technical issues such as trade-offs in device designs. Unfortunately, these trade-offs are so intimately involved in the scientific aspects of both the designs and the research results that they require a genuinely scientific analysis. As a consequence, these parts of the report prove to have little value either as description or as analysis. The various trade-offs are very inaccurately represented, and the simple cost projections based on examination of some previous devices are similarly wrong. The Office of Energy Research is preparing a detailed analysis of the report's contents, which will be available to you and the Congress in late November.

This letter will concentrate on the dominant aspects of cost issues raised in the report, and will close with a few comments on the separate one-page Summary of the report. The report's treatment of several other issues remains troubling: the physics motivation and goals of the SSC, the impact of the project on industrial technologies, trade-offs in R&D investments in basic science, and the role of international collaboration in maintaining the health of U.S. science. In the discussions of these topics we still find serious inadequacies, flawed logic and imbalances. However, such matters will be discussed later in the detailed analysis.

Celebrating the U.S. Constitution Bicentennial — 1787-1987

Cost Estimation Procedures

The report proposes to evaluate the Department's cost estimate of $4.4 billion in two ways: by an examination of large-scale component costs (called "technical analysis" in the report) and by comparison with other recent DOE accelerator projects. The component cost examination will be discussed in the full analysis, since the conclusions made by CBO are not dramatic. The lower bound derived by CBO for the SSC costs is within the Department's stated uncertainty (10%) of costs for the project, while the upper bound is only 6% higher than the Department's upper estimates. We regard this as CBO's concurrence in our estimates.

The second procedure is simplistic: the average of cost overruns for the four most recent accelerator projects is applied to the SSC. Two of these projects came in at cost; by CBO's numbers, one increased about 60%, while the fourth more than doubled its initial estimate. Thus the "average" increase predicted by the CBO for the SSC comes to a rather large factor of 46 percent.

The logic of this approach is essentially invalid. The most surprising aspect of this procedure is the assumed application of statistical analysis to a very small sample size -- four. For example, the three machines built previous to the four selected all came in at or below cost. The choice to exclude these devices from the calculation is arbitrary and unexplained. More importantly, two of these three, Fermi National Accelerator Laboratory (Fermilab) and Stanford Linear Accelerator Center (SLAC), probably provide the best analogies to the SSC, since they were also built from the ground up. Like the SSC, SLAC and Fermilab use previously tested methods, and achieved increases in scale over then-existing machines comparable to SSC's increase over Fermilab. They are the best analogies available.

The report ignores these relevant examples. It also ignores the fact that this "historical analysis" would predict a 46% cost growth for any new accelerator. To show some relevance to the SSC, the Summary simply concludes that SSC has a high risk of cost increases because it is "much larger than those projects [TEV and Energy Saver] and has many more components, whose cost has escalated substantially with previous accelerators." This last reference is vague. If "previous accelerators" means ones before TEV and Energy Saver, the statement is simply not true. When the components did not require extensive development, as with Fermilab and SLAC, there was no escalation. As for Tevatron I and the Energy Saver, both required new technology developments, and those developments cost more than had been expected. But because of the success of those devices, the SSC does not require major component development, and consequently can be costed with confidence. The currently operating Tevatron and Energy Saver facility is in effect a prototype for SSC, and is providing valuable operational experience unique in the history of accelerators.

For both of these devices, the impact of required R&D is clearly shown in the cost increase. The TEV I project, in fact, did not involve construction of a superconducting accelerator, but the building of a new antiproton source using conventional magnets. When the R&D led to a completely new solution for this source, different equipment also had to be bought, so total costs were driven by the changed design. The change in cost was separately approved by Congress before any construction of the anti-proton source began; and the TEV-Saver combination now operates at three times its original luminosity specifications. The Energy Saver contained 40% of its initial budget estimate in R&D, and virtually all the cost growth occurred in the R&D (the report's statement about large cost growth in components on page 50 is contradicted by its own cost figures on page 47). Since the SSC has been designed to use existing technology, its R&D budget is only about 6% of the total cost.

Two other points in the cost analysis are worth mentioning.

First, there are no hidden costs for detectors or further detector development, as the report suggests. The project contains an excellent complement of detectors for the currently planned operations. Later, as physics results are followed up and new experimental ideas considered, new detectors may be proposed. The additional detector costs cited by the report are not requirements of the basic project, but rather a long-term list of possible future detector candidates developed by the Central Design Group.

Second, the Department's estimates do not include offsetting costs from non-federal sources such as foreign or state contributions. The language of the report (page xii) suggests that $1.8 billion from non-federal sources will be necessary to meet the Department's cost estimate, which is not true. Any such contributions will lower the federal cost.

Alternatives to Building the SSC

The second major aspect of the cost issues is the comparison of planned SSC cost and scientific performance with predictions of the cost and scientific performance of possible alternative accelerators. The direct comparison appears in Summary Table 2, "Comparison of Major Future Accelerators", where the SSC is compared to two notional devices -- an intermediate energy electron-positron linear collider, and a large hadron collider (LHC) that is being discussed as a future possibility at the European Organization for Nuclear Research (CERN).

The report states that the SSC "is by far the most expensive of the near-term options." This conclusion is fundamentally flawed, since there are no other near-term options with cost estimates that are more than guesses. The SSC has a complete design with thoroughly defined cost estimates that have been thoroughly reviewed, is technically feasible with existing technology, and has been fully approved by the Administration. The LHC, by contract, has parameter lists, sketchy designs and major technical hurdles to overcome; detailed design work has not even been approved by

CERN. The electron-positron collider is in an even more primitive state. It does not have a fixed, achievable set of parameters worked out, partly because its technical feasibility remains to be demonstrated. A conceptual design report could not be prepared for at least three years.

Electron Positron Collider

This idea looks impressive and inviting in Summary Table 2, though the report does note that the $1.0-$2.0 billion cost estimate contains a high degree of uncertainty. The report bases its discussion of this option primarily on the report of a panel chaired by Cornell's Dr. Robert Siemann. Yet Dr. Siemann wrote Mr. Webre, stating that the CBO report "gives an unrealistically optimistic technical appraisal and cost estimate" for this collider. The Siemann study was conducted to assess how cost and design for such a device would influence one another, not to make an actual estimate; consequently, Dr. Siemann advised Mr. Webre that "it is not appropriate to use our work in the way you have." He quoted his report's real conclusion, directly at variance with the CBO's representation: "While there is hope that the cost of a suitably optimized collider might be less than that of the SSC, it might also be substantially more."

LHC

The report seems to support the idea of joining CERN to develop the LHC, yet does not fully discuss the technical risks and difficulties associated with the project. For example, it is not known if the proposed magnets can even be built: They are of a difficult design that is beyond the capability of current technology. The high collision rate discussed by the devices's proponents would make current detector technology unusable. Thus, considerable R&D still needs to be done to determine if a high-performance LHC is even feasible. With such extensive R&D remaining, cost estimates could be unreliable even if a design existed, as CBO's own case argues; but since no detailed design does exist, the cost risk is very high.

In terms of performance, the LHC would be much inferior to the SSC. The LHC would have fewer detectors than SSC and would share the tunnel with the current CERN accelerator, Large Electron Positron (LEP), which means LHC could not operate when LEP is on. In all, availability of LHC detectors to U.S. physicists would be below those of the SSC by a factor of 10 to 20.

Finally, the physics output of LHC would be vastly lower than that of the SSC. Consider the crucial case of production of Higgs particles with a mass near 1 TEV, a principal motivation for both LHC and SSC. To achieve a production rate of such particles similar to SSC's, the LHC would need a collision rate so high that (as suggested above) it would require an entirely new detector technology; and the background rate obscuring the particles would also be raised significantly above SSC's. If LHC is designed with a lower count rate, one appropriate to the already sophisticated detector technology of SSC, the LHC would take more than 20 years to produce the Higgs particle results that SSC could obtain in one year. The report's statement (on page xv) that "Both the SSC and the Large Hadron Collider would provide more than enough energy to study the phenomena in which high energy physicists are interested" is wrong; in fact, it is contradicted within the report itself on pages xviii and 65.

Summary

The CBO report attempts to present a balanced assessment of the budget and scientific risks and benefits of the SSC. While the component cost analysis is in substantial agreement with the Department, the cost analysis by historical analogy is, in its current form, essentially meaningless. In addition, the analysis of alternatives contains significant factual errors and internal inconsistencies; and the report's summary presents an unbalanced selection of the diverse material in the body of the report.

The single-page press-release Summary is even more imprecise and unbalanced, particularly in the way it casts comparisons. For instance, in discussing the LHC and electron-positron collider, it says that "these alternatives are expected to answer the 'next-step' questions in high energy physics." In the first place, this language suggests that they are expected to be built, regardless of whether the SSC is built; however, there is no firm proposal to build either, and no convincing case that either could be built. In the second place, it suggests that the other machines could answer the same questions that SSC could answer, which is untrue. The SSC design values were chosen to provide confidence in answering those questions; the other two devices would have significant probabilities of failing completely.

The next sentence in the single-page summary is a critical one; it says that "the next Congress will be asked to decide whether the added scientific value and lower technical risk are worth the extra cost of about $3 billion to $4 billion to U.S. taxpayers." The problem with this statement is that it fails to acknowledge the extensive cost risks of the alternatives; it presents as fact savings estimates which are both highly optimistic and unsupportable. It frames for the Congress a choice among false alternatives.

The CBO component cost analysis revealed nothing not already carefully considered, and was in fact based on the Department's own various studies. Thus, the net effect is to reinforce our previous judgments on cost and risk. The Department continues to believe that all evidence demonstrates compellingly that the Super Collider can be built within our previously announced cost estimate of $4.4 billion.

Sincerely,

Robert O. Hunter Jr.

Robert O. Hunter, Jr.
Director
Office of Energy Research

CONGRESSIONAL BUDGET OFFICE
U.S. CONGRESS
WASHINGTON, D.C. 20515

November 17, 1988

Dr. Robert O. Hunter, Jr.
Director
Office of Energy Research
U.S. Department of Energy
Washington, D.C. 20585

Dear Dr. Hunter:

I am writing in response to your comments on the Congressional Budget Office (CBO) report, Risks and Benefits of Building the Superconducting Super Collider. I would also like to reiterate our appreciation for your efforts in reviewing the earlier drafts of the report. The report was substantially improved by those comments. Indeed, I believe that a careful reading of the report would reveal incorporation of nearly all of the points made in your letter.

I suspect that part of your continuing disagreement with the report stems from the different roles and perspectives of the Department of Energy (DOE) and CBO. We recognize that DOE is responsible for developing policies that are meant to advance high-energy physics research. I hope that you will recognize that CBO's role is to analyze, as objectively as possible, not only the route chosen by DOE, but also the range of responsible alternative opinion.

A good illustration of this difference can be seen in the example cited in your letter with regard to the Large Hadron Collider (LHC). While you may not care to view this instrument as an alternative to the SSC, such a proposal has been made by reputable physicists, and in our judgement deserves public attention. I believe that the report accurately portrays this option as one of less scientific capability than the SSC (as indicated by its lower mass reach), and much greater design risk due to technological factors. We also point out that the preliminary cost estimate is constructed on the basis of reasonable assumptions, but that it is not as solid as that of the SSC. With all that said, we are not persuaded by DOE's comments that the LHC is not an alternative to the SSC since many reputable physicists, including several winners of the Nobel Prize in Physics, serve on its planning committee. As you well know, CBO is in no position to resolve a dispute among reputable physicists. As carefully as we can, we attempt to report all sides of such issues, with appropriate caveats and critical analysis where needed.

I hope this letter clears up some of our differences. I look forward to your detailed comments and am sure that they will serve to further illuminate the science budget debate in which we are engaged. Please contact me if I can be of further assistance.

Sincerely,

James L Blum

James L. Blum
Acting Director

ORGANISATION EUROPÉENNE POUR LA RECHERCHE NUCLÉAIRE
EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH
Laboratoire Européen pour la Physique des Particules
European Laboratory for Particle Physics

CERN
CH - 1211 GENEVE 23

Prof. Carlo Rubbia

Téléphone / Telephone:
Direct: (022) 832282
Central / Exchange: (022) 836111
Télex / Télex : 419000 CER CH
Télécopie / Telefax : (022) 824439

Notre Référence / Our Reference:CR/dS/2323/chR/

Mr.
James Blum Esq.
Acting Director
Congressional Budget Office
House of Representatives
Washington, DC 20515
USA

Geneva, November 18, 1988

Dear Mr. Blum,

I am writing to you after being informed of the letter sent to you by Dr. Hunter Jr., Director of the Office of Energy Research of the DOE summarizing the DOE's views on the recent CBO Report "Risks and Benefits of Building the Superconducting Super Collider".

The purpose of this note is neither to give my own comments on an official US report discussing of a US proposal nor to make any objection on a facility which always appeared to me as being a potential asset to High Energy Physics world-wide, as for instance I have publically stated at the occasion of several US Congressional testimonies in which I have served. Here at CERN we are very happy of the increased number of users coming to Geneva amongst which there are more than 300 US scientists, but are concerned by the fact that if no large facility is built in the US, our Laboratory may have problems to house many more users from its non-member States. We have been positive to the SSC' initiative and if I react to Dr Hunter's letter it is because I feel bound to correct some inaccuracies :

1.—The LHC project has been carefully studied, its characteristics have been detailed in several technical reports, well known to the US physicists and presented to the CERN Council in CERN/1658, "REPORT OF THE LONG-RANGE PLANNING COMMITTEE TO THE CERN COUNCIL" and to a large number of Conferences, workshops etc. We have at present at CERN a manpower investment which is quite comparable to the one of the Central Design Group operated at Berkeley by DOE coupled with a very unique expertise for having pioneered the whole field of hadronic Colliders, first with the ISR operated as early as 1971 and the CERN proton-antiproton Collider which has started operation as early as 1981.

2.— It is clear that the LHC will have a collision energy lower than the one of the SSC. This is however reversed as far as collision rate (luminosity) is concerned. Many physicists think that the latter is at least as important for the interest of physics and the discovery potentials of the machine. It is our intention to bring forward a program which is complementary and not alternative to the one than the United States is proposing. It is natural therefore that there should be differences. This does not make under any circumstance our project "inferior", but only "differently optimized".

3.— The cost of the project is estimated at about 1 Billion Dollars (in the tradition of CERN, which contrary to what your report quotes for some DOE projects, has always been respected when contingencies were given and stayed within a 20 % margin when no contingency was given, such as for LEP). Tunnel, injector and all Laboratory infrastructure already exist , and so is in house manpower and expertise.

4.— We know that the magnets can be built and so do the colleagues of the Central Design Group of the SSC (see as an example of the closeness of the collaboration efforts between the US and European communities the annexed copy of the recent letter from Dr. M. Tigner, Director of the Group to Dr. R. Perin who is in charge of the prototypes of the LHC project) ; actually an effective cooperation has been set up with industry, which means several months (if not years) of advance compared to SSC magnets..

5.— The CERN Council, the highest authority of CERN in which each Government of the fourteen Member States have Representatives has formally invited the United States and other Countries to join in the realization of LHC, without any presumption on the technical realizability of the SSC.

I leave to Dr Hunter the responsibility of his opinions about the respective physics outputs of LHC and SSC. As an Experimental Physicist I feel that I should remain modest and careful about any prediction in this field. I know that history only seldom repeats itself, however I cannot avoid wondering: how would Dr. Hunter have compared ten years ago the performance — by then only predicted — of ISABELLE and of the CERN proton-antiproton Collider ?

Sincerely Yours,

Carlo Rubbia

Carlo Rubbia
1984 Nobel Laureate in Physics

Copies:

—Dr. Robert O. Hunter, Jr., Director of the Office of Energy Research DOE
—Professor Francis Low, Chairman HEPAP

OCT-25-'88 11:23 ID: TEL NO:88965053 #011 P02

RECEIVED
26 OCT 1988
NR. 014912

Superconducting Super Collider
Central Design Group
c/o Lawrence Berkeley Laboratory, MS 90-4040
Berkeley, California 94720
(415) 486-4772 • (FTS) 451-4772
Telefax (415) 486-6796

October 25, 1988

Professor R. Perin
SPS Division
CERN
CH-1211
Geneva 23, Switzerland

Dear Romeo:

Many thanks for the time you spent with me last Wednesday. I was very pleased to learn of the great progress you are making and learned a great deal from our discussion. Of very particular interest is your work on the soldered cable. We are very keen to pursue that line also and are now studying the eddy current sextupole and magnetization implications of the solder. Dr. Rae Stiening at the CDG is studying these matters and would very much appreciate an opportunity to communicate with the person(s) in your group who are expert in these matters. Could you suggest name(s) with bitnet addresses or telephone numbers? Rae's bitnet address is RAES@LBL and his phone is 415 486 6309.

As you know, we are also pursuing your idea of horizontal support for the collars by introducing prestress into the yoke through the outer shell. I would hope that we could keep up correspondence on that subject also. Indeed, if you are so inclined, we could perhaps think of a systematic way of exchanging information that would be efficient. In that line, could you consider a visit to Berkeley sometime late this year or early next year to discuss this and to spend some time advising us on the SSC magnet design? If that would be a possibility, and I very much hope it is, please let me know and I could send a formal invitation. Naturally, we should be delighted to pay your travel expenses.

Sincerely,

Maury

Maury

MT:xm

JAN 0 3 1989

MEMORANDUM FOR RECORD

FROM: Robert O. Hunter, Jr.
Director
Office of Energy Research

SUBJECT: Carlo Rubbia...Letter of November 18, 1988, CBO/SSC

Dr. Rubbia's letter is notable both for what it says and for what it avoids saying. The points are selected to create the impression of stronger argument than is actually made.

1. Dr. Rubbia refers to a report of a long-range planning committee, not a design study, as evidence of the mature status of the LHC. This is consistent with our own description: The devices is in the long-range planning stage, being discussed by committees and workshops. The SSC was at this stage about 4-5 years ago. The value of CERN's pioneering of hadron colliders is not disputed; but it has nothing to do with the present status of a particular device, the LHC.

2. Dr. Rubbia does not dispute any of the numerical estimates presented in my letter to Mr. Blum. If the objective is to find a Higgs particle in the vicinity of 1 TEV, the LHC might produce relevant events at roughly the rate of the SSC by taking advantage of the possibility of a very high collision rate; but this rate would create a background problem and would require the creation of an entire new detector technology. Thus, that choice opens up the LHC to both technical risk and the possibility of extensive cost growth. With the most advanced detectors we can currently build, such as those planned for SSC, the LHC's energy and collision rate would, as I said previously, takes 20 times as long as SSC to produce equivalent results. And, of course, for a 2 TEV particle the LHC will essentially not produce results. This is more than merely being "differently optimized." The problem is that LHC must be designed to be productive within the constraint of an existing tunnel, and that places severe limitations.

3. Dr. Rubbia fails to mention that CERN cost estimates routinely do not include labor costs, while U.S. DOE estimates do include them. Thus the CBO estimate of "one to two billion dollars" is more appropriate. These cost estimates remain rough because no detailed design study has been done, and because technical problems (magnets and possibly detectors) remain to be solved. Congress would never consider actual construction funds for a U.S. project of any significant size, far less one in the billion-dollar range, without an engineering design and costing.

4. Dr. Rubbia asserts that he knows that the magnets can be built, essentially a statement of faith. He does not specifically challenge the assertions in my letter. These points were provided by me by the SSC Central Design Group, suggesting that he overstates their confidence. The letter Dr. Rubbia provides only demonstrates that the CDG is working together with CERN, which is certainly commendable; it says nothing to suggest that the CDG shares Dr. Rubbia's faith. The fact remains that the magnets being considered for LHC have requirements of size and complexity that are beyond what anyone has done, while the SSC magnet designs do not force an extension of technological capability.

5. Dr. Rubbia says that the CERN Council has invited the U.S. to join in "realization of the LHC." He does not say that CERN has decided to build the LHC, that it has committed construction funds, or that it has authorized production of an engineering design. Invitations such as the one he refers to are frequently proffered by organizations and nations for projects that lack formal commitment.

As to Dr. Rubbia's closing paragraph, it would appear to speak for itself.

Attachment:
C. Rubbia ltr of 11/18/88

February 10, 1989

Mr. James L. Blum
Acting Director
Congressional Budget Office
U.S. Congress
Washington, DC 20515

Dear Mr. Blum:

I am writing to you again to forward the Department of Energy's response to the Congressional Budget Office's (CBO) report, "Risks and Benefits of Building the Superconducting Super Collider." On November 9, 1988, I wrote to you summarizing the Department's views on the CBO report, particularly with respect to our strong disagreement with the CBO's historical cost comparison analysis of the SSC. The enclosed response is a more detailed discussion of our views, not only as to cost estimates, but also to the CBO's presentation of possible alternatives to proceeding now to build the SSC. Here too, we must take strong issue with the CBO analysis of alternatives, particularly with respect to the viability of considering the Large Hadron Collider (LHC) and an electron-positron linear collider as meaningful options to proceeding now with the SSC.

Your letter of November 17, 1988, concedes that the proposed LHC option has less scientific capabilities than the SSC, entails much greater design risk, and employs preliminary cost estimates constructed merely on the basis of reasonable assumptions. Then you contend that LHC is an alternative to SSC simply because "many reputable scientists, including several winners of the Nobel Prize in Physics, serve on its planning committee." Surely, the fact that respectable people are willing to spend time on the planning committee for a device says nothing about its specific prospects or relative status compared to the SSC or to any other device. It seems inconsistent to us for you to simultaneously claim that the LHC is a feasible alternative to SSC but that any judgment is beyond your expertise because of a "dispute among reputable physicists." But the physics issues are not in doubt at all: everyone agrees that the LHC would be much less capable than the SSC. The disagreement is over far more familiar matters of practicality, and over the utility of the decision process itself.

Before new accelerator projects are approved, the Department engages in a process that includes extensive in-house and independent analyses (for example, those of the formally chartered High Energy Physics Advisory Panel). In addition to this programmatic process, new projects must undergo rigorous budgetary reviews within the Department itself and by the Office of Management and Budget. The SSC project has successfully completed all of these analytical processes, producing confident predictions of cost, schedule, and capability. The LHC has been through none of them; indeed, there is not yet sufficient design and cost information on the LHC to accomplish these analyses. Yet the CBO continues to suggest that it is responsible to compare the two proposed projects as alternatives to the same goal. Your suggestion that CBO's function is simply "to report all sides of such issues" is disturbing since decision makers must have access to complete and accurate information in order to make an informed decision.

Timely construction of the SSC will give the United States an important and unparallelled scientific instrument and will keep our country on the forefront of scientific, educational and technological progress. The chance to make such an outstanding investment in America's future is a rare and historic opportunity.

Sincerely,

original signed by

Robert O. Hunter, Jr.
Director
Office of Energy Research

Enclosure

CONGRESSIONAL BUDGET OFFICE
U.S. CONGRESS
WASHINGTON, D.C. 20515

February 27, 1989

Dr. Robert O. Hunter, Jr.
Director
Office of Energy Research
U.S. Department of Energy
Washington, D.C. 20585

Dear Dr. Hunter:

I have received your letter forwarding the Department of Energy's detailed response to the Congressional Budget Office report on the Superconducting Super Collider (SSC), and note your strong exception to CBO's analysis of the alternatives to the SSC. I doubt that it is possible for us to agree. I feel obliged, however, to point out several misreadings of the CBO analysis in your letter and response, because such misrepresentations serve only to degrade the quality of discussion on this important issue.

We stand by our assessment of the scientific and technical merits as published in our October report, and categorically reject the implication in both your cover letter and page two of your response that we did not advise the Congress of the current status and risks associated with the Large Hadron Collider (LHC). In our letter of November 17, 1988, we stated that we took the LHC seriously because the physics community did. Your February 10, 1989 letter characterizes the European Organization for Nuclear Research (CERN) study efforts as just a committee of interested physicists "willing to spend time on a planning committee." In fact, as noted by Carlo Rubbia, the current Director General of CERN, in his letter of November 18, 1988 (of which you were sent a copy), the CERN Long Range Planning Committee did make a report to the CERN Council, the highest authority in CERN, on the LHC, just as the HEPAP physicists make reports to DOE. This institutional context lends greater credence to CERN's efforts than you acknowledge. As Carlo Rubbia stated in his letter, their design manpower commitment at CERN is comparable to that of the Central Design Group (CDG) at Berkeley. The fact that SSC is further along in the design and governmental approval process does not make the LHC an inappropriate instrument for the next generation of physics questions.

Another example of misreading the CBO report regards the superconducting magnets. Contrary to the implication in the DOE response, the full discussion of the magnet problems on pages 62 and 63 of the report presents the many short magnet tests as evidence of DOE progress. Those pages describe the consequences of the superconducting magnets' problems and note how DOE has dealt with such problems in previous accelerators. The superconducting magnets are of great interest to the

Congress, not only because they are critical to the SSC and as yet unproven at the size required, but because problems with much less ambitious magnets caused the termination of the Isabelle program after approximately $220 million had been spent. The Congress needs to know not only where you are, but how much further you have to go before success is assured. Until those assurances are possible, it might reasonably decide to delay further commitment.

Similarly, your response makes it seem as if our report ignores the previous spin-offs from high energy physics. In fact, a history of such spin-offs is the focus of Appendix A in our report. The issue in deciding whether to fund the SSC in a tight budget is not spin-offs from high energy physics in general, but rather the spin-offs from building the SSC. The question is not whether government spending in general or science spending might produce spin-offs. It is whether the SSC in particular can claim some extraordinary or unique contribution, and whether that contribution, if it exists, might not be realized more efficiently through direct funding.

We focused on the superconducting magnets because they account for 40 percent of the construction cost of the project, and many of the other technical components, such as large parts of the computer network, will use "off-the-shelf" technology. The CBO report acknowledges the contribution of Fermilab and the SSC's research and development to the superconducting magnet industry. Given this history, though, it is not clear that solving the technical problems encountered in increasing the size and power of superconducting magnets to 17 meters and 6.6 tesla is relevant to the needs of the commercial users of low temperature superconductors, which use much smaller, and less powerful magnets with much less stringent requirements. The SSC's research and development has improved superconducting cable, but this improvement did not require building the SSC; even if the SSC is never built, the cable is now better than it was before. Furthermore, cable can be improved with a small research program, and does not need a massive construction program.

I am sympathetic to the belief expressed in your report that the SSC will create spin-offs, motivate a new generation of scientists, and enhance national prestige. Unfortunately, these beliefs are also held strongly about many other large, expensive projects involving advanced technology. As a result, they are of no help in deciding which projects should be funded in the face of scarce budget resources. To make those choices, the Congress needs dispassionate information on the potential merits and drawbacks of each alternative. CBO has attempted to describe both the good and the potentially bad points of the SSC and its alternatives. I would hope that we might agree that there are some potential drawbacks to the SSC, as well as positive features, and that there is something good to be said about its competitors, regardless of how these pluses and minuses are weighed in the final balance.

Sincerely,

W. David Montgomery
Assistant Director
Natural Resources and Commerce Division

RISKS AND BENEFITS OF BUILDING THE SUPERCONDUCTING SUPER COLLIDER

The Congress of the United States
Congressional Budget Office

For sale by the Superintendent of Documents, U.S. Government Printing Office
Washington, D.C. 20402

CBO Publication

SUMMARY

October 1988

CBO STUDY ON THE SUPERCONDUCTING SUPER COLLIDER

The United States is currently considering building the Superconducting Super Collider (SSC) to support future research in high-energy physics. The SSC is the largest and most powerful particle accelerator yet proposed worldwide and is intended to achieve major advances in the study of the basic nature of matter. A special study by the Congressional Budget Office (CBO), *Risks and Benefits of Building the Superconducting Super Collider*, prepared at the request of the Senate Budget Committee, finds that the SSC could cost more than the $4.4 billion (in fiscal year 1988 dollars) estimated by the Department of Energy (DOE). CBO's technical and historical analyses of the DOE estimates suggest that costs might rise by as little as 4 percent (well within DOE's own margin of error) or by as much as 46 percent (the average real cost increase for particle accelerators built by DOE during the 1980s).

In 1988, high-energy physics funding accounted for 6.6 percent of federal spending on basic science research. Construction of the SSC would double the share of research spending going to high-energy physics for about six years, assuming no real increase in funding for other basic science. In contrast, the 2,200 high-energy physicists and 600 Ph.D. candidates in high-energy physics account for just 3 percent and 0.6 percent, respectively, of all active basic research scientists and Ph.D. students in science.

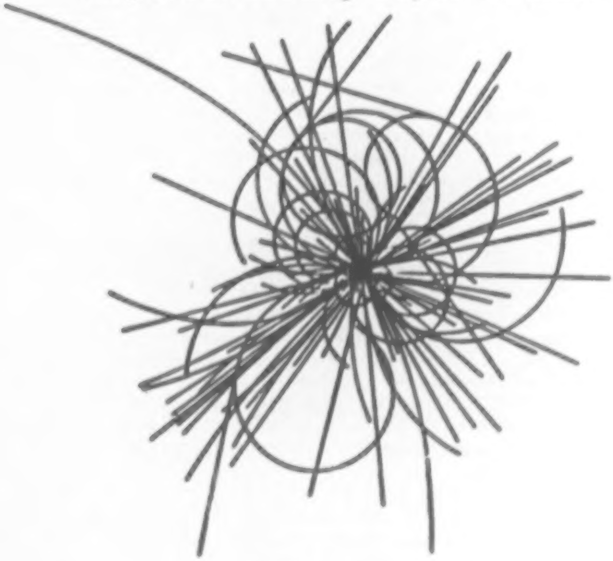
Alternatives to the SSC include joining the European Organization for Nuclear Research (CERN) in the planning and construction of their proposed new accelerator (the Large Hadron Collider) or building an advanced electron-positron linear collider. While these alternatives are expected to answer the "next-step" questions in high-energy physics and could save the Congress as much as $4 billion, both are technically riskier and less capable scientifically than the SSC. The next Congress will be asked to decide whether the added scientific value and lower technical risk are worth the extra cost of about $3 billion to $4 billion to U.S. taxpayers. Another option available to the Congress is to defer the decision while DOE improves the technology for the SSC's superconducting magnets and continues research on the alternative particle accelerators.

The Office of Intergovernmental Relations is CBO's Congressional liaison office and can be reached at 226-2600. For additional copies of the report, please call the Publications Office at 226-2809. Questions regarding the analysis should be directed to the author, Philip Webre, of CBO's Natural Resources and Commerce Division at (202) 226-2940.

CONGRESSIONAL
BUDGET OFFICE
Second and D Streets, S.W.
Washington, D.C. 20515

Department of Energy

Response to the
Congressional Budget Office
Report on the
Superconducting Super Collider



February 1989

RESPONSE TO THE CONGRESSIONAL BUDGET OFFICE REPORT
"RISKS AND BENEFITS OF BUILDING THE SUPERCONDUCTING SUPER COLLIDER"

Summary

THE SUPERCONDUCTING SUPER COLLIDER

In order to maintain our leadership position in the scientific field of high energy physics and to continue reaping the extraordinary scientific, economic, educational, and technological benefits associated with frontier basic research, the Department of Energy (DOE) is proposing the construction of the Superconducting Super Collider (SSC). The SSC is a meticulously planned undertaking, based on mature technology, that is indispensable to the timely progress of science. In contrast to the alternatives cited by the Congressional Budget Office (CBO), the SSC can be undertaken today with confidence in its schedule, performance, cost, and scientific potential.

BUDGETARY RISKS AND BENEFITS

The CBO report evaluates the Department's cost estimate of $4.4 billion (1988 dollars) in two ways: by an examination of large-scale component costs (called a "technical analysis" in the report) and by a "historical" comparison with recent Department of Energy (DOE) accelerator projects. The CBO's technical analysis of the SSC cost estimate projects a small increase in cost over the Department's estimate, not inconsistent with the Department's present 10 percent estimate of uncertainty.

Historical Cost Comparison

The historical comparison, however, is misleading and invalid. It looks at the budget of four previous accelerator projects, two of which came in at cost and two that went over budget. Averaging these four projects together leads to a suggested "potential" overrun of 46 percent for any accelerator project, no matter how carefully designed or conservatively costed. This comparison ignores three earlier accelerator projects which all came in at cost, and it excludes the very projects that provide the best analogy for the SSC--the initial construction of two laboratories, Fermilab and Stanford Linear Accelerator Center (SLAC). Like the SSC, these projects were built from the ground up utilizing existing, proven technology.

The two projects included in the CBO analysis which experienced overruns both required new technology not previously tried in accelerator construction. The development and construction of superconducting magnets for the Energy Saver, and the antiproton cooling system at Tevatron I, were new and highly important steps forward in accelerator science. In many ways, the Energy Saver serves as a prototype for the SSC. The SSC does not depend on new, untried technology and, therefore, the SSC can be costed with confidence. The SSC has also been preceded by a 5-year research and development (R&D) and design program, receiving more extensive study and review than any previous accelerator at this stage of its development. Furthermore, the SSC project budget contains a 21 percent contingency allowance on construction costs and a significant allocation for further R&D, substantially reducing the risk of cost overruns.

The summary's conclusion that the SSC has a high risk for cost increases because it is "much larger than those previous projects (Tevatron I and Energy Saver) and has more components, whose cost has escalated substantially with previous accelerators," is simply wrong. When components did not require extensive development of new technologies, as was the case for the construction of Fermilab and SLAC, there was no escalation. A more comprehensive historical analysis would look at the construction of all seven DOE accelerator construction projects completed since 1965. As discussed in

Chapter III, the total cost overrun was only 1.2 percent, even including the need for new technological developments in the two projects highlighted by the CBO. 2

SHARING THE COST OF THE SSC

The Department's $4.4 billion cost estimate for the SSC does not include any State or foreign contributions. However, foreign contributions and participation are commonplace in the high energy physics field, especially in the area of experimentation. The Tevatron Collider Detector at Fermilab received over $15 million (30 percent of its total cost) in contributions from foreign participants. A single detector (L3) at CERN is being constructed with about $100 million worth of help from countries that aren't members of CERN, including the U.S. Over 300 American scientists conduct research at CERN, their expenses being paid primarily by sponsoring American institutions and agencies. The Indian Government has already signed a Science and Technology exchange agreement, indicating a possible in-kind contribution of $50 million over 10 years to the SSC. Officials from many other countries, including Italy, West Germany, Canada, and Japan, have indicated interest in participating in the SSC project. A significant flow of scientists and contributions to the SSC are expected, and this will lower the cost to the federal government. Substantial cost sharing is also anticipated from the host State, though the details have yet to be worked out.

ALTERNATIVES TO THE SSC

The CBO's comparison of cost and scientific performance of the SSC and two alternatives, the Large Hadron Collider (LHC) at CERN and an intermediate-energy electron-positron collider, is incomplete and misleading. These options are on very different footings, technologically and scientifically. It is important to have clearly in mind not only the cost, but what can be purchased for the price. The SSC has a complete design and has been meticulously reviewed for cost and technical feasibility by both DOE officials and independent experts. The LHC is less capable than the SSC, having lower energy and far less capacity to serve experiments. The LHC has the outline of a design, from which it is possible to guess the cost but not to guarantee the performance. There is not yet a full conceptual design for such a machine, as was done for the SSC in 1986, and the required magnets are of a special new kind that have not yet been built. Many physicists do not think it is possible to make detectors for the high collision rate required for the LHC to approach some of the capabilities of the SSC. The second option is simply not real: we do not, at present, know how to build an intermediate-energy electron-positron collider.

The CBO report does not adequately acknowledge the number of experiments each of the options could support: SSC, 4–6 major detector facilities; LHC, 2–3 such facilities which can only operate one-half of the time because running time must be shared with the existing accelerator, LEP; an electron-positron collider, 1 experiment. The number of experiment-hours available at the LHC would be about one-quarter that at the SSC, and U.S. physicists might have access to one-quarter to one-third of that amount, clearly far below that required for U.S. research needs. The SSC would maintain a more diverse experimental portfolio than the LHC, in addition to being more powerful.

There is no credible basis for an electron-positron collider cost estimate. In late 1985, the High Energy Physics Advisory Panel's (HEPAP) Subpanel on Advanced Accelerator R&D and the SSC concluded that "while there is hope that the cost of a suitably optimized electron-positron collider might be less than that of the SSC, it might also be substantially more." Many capabilities, in particular the required beam quality, have to be greatly improved beyond today's technology. A JASON study group reported in October 1985 that "the proposed technologies are so new, and the specifications which must be met to produce a multi-TeV particle physics machine are so stringent, that in our opinion 20 years are likely to pass before any of them are ready for an accelerator of more than SSC capability. In fact, success cannot be guaranteed at this point." Nothing has happened since that time to change these conclusions. The same uncertainties that affected those assessments apply to a more modest linear collider. A viable cost estimate for an intermediate-energy linear collider cannot be made until the basic technical issues are resolved and the major components designed and engineered. Until then, an electron-positron collider cannot be presented as a viable option, except as one with a highly uncertain future time frame, an unknown cost, and a very high instrument risk.

A more accurate representation of CBO Summary Table 2 is shown below:

TABLE A. Comparison of Major Future Accelerators

	SSC	LHC	Electron-Positron
Estimated total cost (billions of FY 88 $)	$4.5 - $5.1*	$2.4 - $3.1	Too early to estimate
U.S. share	60% - 100%	25% - 33%	
Date of Detailed Conceptual Design	1986	about 1990	1992 or later
Completion date	1998	late 1990's	Too early to estimate
Energy	40 TeV	16 TeV (Maximum)	0.5 to 2 TeV
Number of experiments	4 - 6	(2 - 3) x 1/2	1
Instrument risk	low	high	highest

* This is the estimate from the CBO table. The DOE estimate is $4.4 billion.

To summarize the table, the LHC would provide 40 percent of the energy with about one quarter the experiment-hours of the SSC. Furthermore, the three projects are in very different states of readiness. The SSC has had extensive design review and is a formal proposal; the LHC is still in the preliminary design and R&D phase; and the electron-positron project is in such an early R&D phase that it probably should not even be included in the table.

Table A also shows that there is considerable potential for non-federal and foreign contributions to the SSC--something not fully recognized in the CBO report. In its Summary Table 2, the CBO report completely ignores any non-federal funding for the SSC, showing the full $4.5 - $5.1 billion as "Estimated Cost to the United States." Similarly, the text ignores non-federal cost sharing and quotes "extra costs of $3 billion to $4 billion to U.S. taxpayers," for the SSC compared to LHC or an electron-positron collider. The report inexplicably assumes substantial cost sharing for the LHC but none for the SSC. The Department is confident that a substantial portion of the cost of the SSC will be borne by the host State and by foreign participants. The $4.4 billion cost estimate for the SSC does not assume any cost sharing. State and foreign contributions will reduce the federal cost.

CONGRESSIONAL OPTIONS

The options in the summary section of the CBO report do not, in fact, provide the Congress with any viable alternatives. Expenditures are emphasized to the exclusion of other important considerations. A crucial point, both for the health of U.S. science and technology and for the standing of the United States in the international community, is whether the U.S. will maintain its historical leadership in a field of such fundamental scientific importance and continue to reap the benefits associated with such a frontier facility.

Deferring the decision to construct the SSC would push up the overall cost of the project with no clear benefit, have a negative impact on America's scientific and industrial communities, and delay the advance of research in a field that has been contributing extensively to society throughout the 20th century. Over the next few years, the construction of other particle accelerators--LEP in Europe and HERA in Germany, followed soon after by UNK in the Soviet Union--will be complete. Without the SSC, these foreign facilities will draw U.S. scientific and technical personnel abroad. Advances in high technology made at these facilities will first benefit the industrial firms working closely with staff at the facilities. Rebuilding the U.S. scientific and industrial base that is currently in place, and needed to support the SSC, would prove exceedingly difficult at a later time.

Build the SSC

The timely construction of the project will not only hold down costs, but will hold together the existing consortium of scientists, educators, technicians, and industries that are required to make this project a reality. Without the SSC, the U.S. high energy physics community would certainly not "continue to flourish" as stated in the CBO report. No benefits are accrued by deferring the project.

The scientific benefits of the SSC, barely discussed in the CBO report, are exciting. The SSC will provide unparalleled insight into the world of elementary particles and into events that occurred during the first moments of existence of the universe. It will help scientists and theorists in their quest to find a unified theory of the particles and forces ruling the cosmos.

The CBO report barely recognizes the technological and social benefits of the SSC. In the case of low-temperature superconductors. It is entirely possible that enhanced performance, such as might emerge from SSC R&D, will be decisive in some areas of application. Magnetic resonance imaging would have new possibilities for increased resolution and the ability to distinguish different elements if the magnetic field strength could be increased in the magnets used. This may become possible with improved superconducting cable.

Join CERN in Building the LHC

This option is premature. LHC is far less capable than SSC; LHC is not an approved project. LHC has no definite cost estimates and has technical challenges that require considerably more R&D. Most importantly, as the CBO report acknowledges, it would not have the scientific capability of the SSC.

Furthermore, the LHC would be built in the same tunnel as the existing Large Electron-Positron Collider (LEP). There will be strong pressure from the scientists using LEP to fully realize the scientific potential of that frontier facility and this would interfere with full utilization of the LHC. Construction of the LHC would have to fit into shut down periods of the LEP, and operations would have to be alternated between the two colliders since both couldn't run at the same time, and both would have to be shut down for maintenance on either facility. The number of experiment-hours of running would be about 1/4 that of the SSC. Finally, the result of this option would be to place all the world's leading high energy physics research in a single tunnel in Switzerland. This can hardly be viewed as a prudent choice either for the field or for the United States and would not satisfy U.S. research needs, nor would it have the desired benefits for U.S. science and industry.

Build an Electron-Positron Collider

Eventually, electron-positron colliders with total energy of 0.5 TeV or more may become very productive scientific facilities, but at the present time, they are simply not realistic options for consideration, even at these reduced energies. The conclusion of the HEPAP Subpanel on Advanced Accelerator R&D and the SSC was that, "the earliest time at which an electron-positron linear collider, if feasible, could overlap the range of physical interest of the SSC is 15 to 20 years." A linear collider with a smaller energy level, one even well below the physics capability of the LHC, still has substantial technological problems. Given the uncertainty in the parameters and technology for such a facility, no firm designs or cost estimates can be prepared for such a collider. Starting such activities is at least 3 years away. The technical uncertainties associated with this collider rank it as the "highest" risk option. The statement from the CBO report that "an electron-positron linear collider with a mass reach approaching 1 TeV...would reach new energy levels for a lower total cost than the SSC" is pure conjecture. In fact, there is simply no technical basis for even a guess today.

Chapter I

Introduction

This chapter of the CBO report is largely a summary of the current situation.

Chapter II

The SSC and the Public Interest

BASIC SCIENCE AND THE PUBLIC INTEREST

Basic scientific research is often unpredictable; we do not know beforehand the technological, economic or scientific benefits it will provide. This is a major reason why the federal government is an appropriate sponsor. History, however, has repeatedly shown the benefits of frontier research. Indeed, it is often the unexpected discovery that makes the biggest impacts.

The CBO report states that the SSC is likely to provide benefits to a limited number of technological fields. Fermilab, the most powerful particle accelerator in existence now, has spin-off benefits in superconductivity, cryogenics, electronics, new computer architecture, and medical technology. The SSC will have similar benefits and has already had spin-offs from the magnet R&D program. In any case, funding only research solely on the basis of immediate practical payoffs would soon put the U.S. at a severe disadvantage in world competition.

THE FEDERAL SCIENCE PORTFOLIO

The CBO introduces the discussion on this topic by stating that "the SSC may very well crowd out other basic science research." This statement assumes a "zero-sum" game. This is not the way Congress or the Administration set budget priorities. The country depends on making investments today, particularly in science, if we are to guarantee our future ability to compete in an increasingly high-tech world. History shows that large science projects need not adversely affect other science.

These investments have always been a small fraction of federal spending by virtually any comparative standard, and have been judged on their merits as contributions to knowledge. Funding of individual large projects in the past, such as Fermilab, has not come at the expense of other research, either in the same field or in unrelated fields.

Figure 1 illustrates the fact that more generally the U.S. is not investing as heavily in non-defense R&D as are some of our competitors, notably Japan and West Germany. The Department believes that the U.S. should not consider R&D as a zero-sum game, but that scientific and technological opportunities should be considered on their own merit and pursued where appropriate.

Many of the statistical comparisons quoted in the CBO report are misleading. For example, the reader may not be aware of the shift of basis in comparing the "6.6% of all federal basic science dollars" going toward high energy physics compared to "3% of all active basic research scientists." Given its nature, high energy physics is mainly and appropriately supported by the Federal Government, while many other basic research scientists are supported by industry, universities, and other institutions. The CBO report leaves the reader with an apples and oranges comparison by mixing different categories. The comparison base used is further confused on page 17 of the CBO report where the National Science Board (NSB) report, Science and Engineering Indicators-1987 (page 274), is quoted as showing "72,200 Ph. D.-level scientists were working in all fields in the United States." The total of 72,200, as shown in the NSB report, is for engineers as well as scientists, in academic R&D as well as basic research.

The report also fails to indicate that the facility will be used by students and scientists outside the field of high energy physics. Students in electronics, engineering and computer science, and scientists in the fields of materials, astronomy, and medicine currently conduct research at high energy physics facilities. Unfortunately, both the CBO report and its one page summary, do not accurately present this data for comparison.

It is certainly true that large experimental facilities do cost more than some other means of pursuing scientific knowledge--much more than those theorists who require only pencil and paper. But experimental data, as well as theory, is needed to make progress, and in some fields (both basic and applied) the frontiers cannot be moved without the aid of expensive experiments.

SPIN-OFFS FROM THE SSC

The CBO report describes the physics goals of the SSC too narrowly and fails to adequately discuss the technological, educational, scientific, and national benefits of building the SSC.

The benefits of past accelerator research have led to advancements outside of the sciences of physics and cosmology. Medical science, computer technology, electronics, and various other high-tech fields have also benefitted. Millions of people have benefitted from such medical advances as proton therapy for treatment of cancer, CAT and PET scans and Magnetic Resonance Imaging. Detectors invented in particle physics are being used in hospitals, allowing doctors to watch the heart and arteries in action. New types of materials are made possible by ion implantation using accelerators. One of these materials is used to make surgical implants (artificial hip joints) 400 times more corrosion resistant. Improvements in semiconductor technology using ion implantation have allowed American computer, electronics, and communications industries to remain competitive in the world marketplace. Important developments in high power radio-frequency systems, ultra-high vacuum systems, and precision beam optics have come from work in particle physics and are being applied in American industries.

Since 1957, 19 American high energy physicists have received the Nobel Prize in Physics--more than any other country. The SSC will serve as a focal point for students of physics, cosmology, medicine, electronics, engineering, computing science, and a variety of other studies, with researchers expected from over 100 U.S. universities. It will inspire and challenge young people to go into fields of science and technology, and it will attract world-class scientists from abroad to work in this country.

A most important point to make is that while past accelerators have, in general, delivered on their promised goals, the discoveries of greatest impact have often not been those anticipated when authorization was sought. The Berkeley Bevatron, conceived to find the antiproton (which it did), is most famous for the mapping out of particle resonances. SLAC, designed to extend the study of electron-proton elastic scattering, had its greatest impact in inelastic scattering that revealed the quark substructure of the proton and has also proved invaluable as a source of beam for storage rings. Similar comparisons can be made for earlier and later machines. The SSC, therefore, has to be seen as fulfilling two roles: first to answer the questions we now know how to pose, and secondly to provide a large reach into the unknown so that fruitful and telling experiments can be carried out over the quarter-century or more lifespan expected for the SSC.

SUPERCONDUCTING MAGNETS

The report asserts that a superconducting magnet industry would be hurt by the dramatic increase in demand to build 10,000 magnets. This is contradicted, however, by the many firms which have expressed interest in participating in the SSC magnet program. Furthermore, the SSC could serve as a catalyst for growth in this industry, and the large quantities of superconducting cable and magnets needed for the SSC could reduce the cost of these items for other applications.

Chapter III

Budgetary Risks in the SSC Project

DOE COST ESTIMATES

The current DOE estimate of $4.4 billion is based on extensively detailed and reviewed cost examinations. The CBO's technical analysis of the SSC cost estimate projects a small increase in cost over the Department's estimate, not inconsistent with the Department's present 10 percent estimate of uncertainty.

CBO's "historical" analysis is invalid. It would project a 46 percent increase in all accelerator projects, no matter how conservative the design and cost estimate. Further details are supplied below.

DETECTORS

The numbers quoted by the CBO report are based on the 1986 cost estimate study for the initial SSC experimental equipment commissioned by the Central Design Group (CDG). The detectors costed were not optimized, and the DOE believes that the overall scope assumed was somewhat more ambitious than necessary for an initial complement of detectors. The study presents preliminary thoughts of a specific group of physicists about what detectors might be useful and desirable for the SSC. As noted in the report, "The detector complement adopted represents an ambitious possible model for the initial set of detectors. Some parts of it may be more representative of what may be expected after a few years of gradual evolution and upgrading of the detectors. There has been no careful optimization of physics output per detector dollar in either the choice or the design of the detectors."

The DOE's best estimate for the initial complement of detectors, including R&D and contingency, is a total of $719 million (FY 1988 dollars). This figure also includes $75 million for off-line computers, as suggested by the CDG study. There is no doubt that more money could be spent on an initial set of detectors; however, the DOE does not believe that this is necessary, and the DOE will manage-to-cost to ensure that there are no serious overruns of the agreed to cost. This technique has been shown to be effective in the past, resulting in cost-effective detectors that yield world-class physics data.

SUPERCONDUCTING MAGNETS

The analysis of budget risks for superconducting magnets in the CBO report summary, and beginning on page 38 of the full report, contains many inaccuracies (see Appendix I for a detailed summary of the magnet program).

The discussion of SSC magnet development on page xvi ("only two of eight prototype superconducting dipole magnets DOE has built so far have been even partially successful") is incorrect. About thirty short (1 to 4.5-meter) model magnets have been built and tested. They have routinely met all the design criteria for SSC magnets and many have reached fields above 9 tesla (about 1.4 times the SSC design field) in low-temperature operation. This demonstrates the mechanical integrity of the basic design. All of the design criteria have also been met in recent full-size (17-meter) magnets. Two recent long magnets have quickly reached the SSC operating field and have been operated above the fields required to demonstrate extra mechanical strength. Thus, at the present time, magnets could be built that meet all design criteria. In practice, the R&D program will go on in order to test limits, to increase the safety margin of certain key parameters over that needed for SSC operation, and to increase reliability and quality control.

The statement on page 40 that "DOE has encountered many problems in its attempt to produce these magnets in a research environment," ignores the fact that steady progress has been made. If there were no problems to be overcome, research would not have been necessary in the first place. The CBO analysis does not take into account the purpose of an R&D program, where devices are routinely constructed in order to determine which of several alternatives works best and is least costly. Contrary to the CBO report assertion, superconducting cable meeting SSC specifications has been successfully produced in U.S. industry, with adequate amounts available for the magnet R&D program, and there are multiple domestic suppliers of the basic niobium-titanium alloy used in the magnets.

The improvements in manufacturing capabilities and technology, envisioned in the Conceptual Design Report (CDR) cost estimate, were reviewed by industrial specialists. The dramatic improvement in the quality of the superconductor projected in the CDR has already been realized. Experience since the publication of the CDR has given no reason to revise the cost estimates. The allowance for rejects after assembly is small because of a "zero rejects" philosophy in which magnet components and raw materials are tested before assembly so that no defective components are used. All superconducting cable will be tested, its dimensions will be tightly controlled, and insulated cable will be tested by the machine that insulates it. The Tevatron experience is the basis for these projections.

It is correct, but misleading, to say (page 41) that the Tevatron magnets were built under the supervision of highly trained and motivated technicians. The Tevatron magnets were built in an industrial-like setting with minimum-wage labor. The supervisors themselves were inexperienced in superconducting technology at the beginning of the project. It is expected that industry will employ similarly motivated supervisors. One function of the industrialization program now in progress will be to transfer to potential vendors the knowledge of the technicians in the national laboratories. The cost estimates for industrial fabrication of the magnets were reviewed by industrialists.

SCHEDULE

As shown by Table B, on page13, the seven high energy physics projects completed since 1965 have generally been on schedule (or even ahead of schedule), the two exceptions being the two projects which employed major new technologies and which also had cost overruns. Since the SSC project does not use new, untried technology, it is expected to be on schedule (or even ahead) as was previously the case when constructing new laboratories (SLAC and Fermilab). The SSC CDR showed a 6 1/2 year construction schedule, but this has already been increased to over 8 years by the assumed funding profile. The present schedule is thus not "tight" or technology limited, but is driven by the funding profile assumed.

CONSTRUCTION COSTS OF PREVIOUS ACCELERATORS

The CBO's cost analysis of previous accelerator projects is an inappropriate model for understanding potential cost problems with the SSC. It fails to take into account the most relevant accelerator projects, the initial construction of the Fermilab and SLAC laboratories. Like the SSC, these were built from the ground up and did not require development of new, unproven technology. The CBO report also ignores the fact that this "historical analysis" would automatically predict a 46 percent cost growth for any new accelerator no matter how conservatively designed or costed. To show some relevance to the SSC, the report on page vii simply concludes that the SSC has a high risk of cost increases because it is "much larger than those projects [Tevatron and Energy Saver] and has many more components, whose cost has escalated substantially with previous accelerators." This is clearly incorrect. The risk of escalation depends on whether the technology is new and untried, not on the number of components. When the components did not require extensive development, as with the original Fermilab and SLAC laboratories, there was no escalation. The SSC pushes the technological envelope in the interest of cost and performance optimization, but does not require the invention of new technologies. The currently operating Tevatron facility is, in effect, a prototype for the SSC and is providing valuable operational experience.

The CBO "historical" analysis ignores the difference between the development of pioneering technology and the application of established technology. A more careful analysis of past cost-estimate performance on high energy accelerator projects is shown in Table B. This table shows all seven projects completed since 1965. This analysis does not try to include the R&D or detectors associated with these projects for several reasons. While the construction projects have always been well defined and a careful separation of cost maintained, this has not been true for the R&D and detector categories. The division between R&D strictly applicable to the project at hand and that more generally useful to advancing the field is very unclear, especially a priori. The exact amount to be charged off against a specific project is, at best, arbitrary and, at worst, misleading even with the best of intentions. The scope and cost of detectors depend on the evolution of the science and of the physics questions having highest priority. In the past, this has evolved as groups of researchers respond to calls for proposals by the laboratory director, who (with his advisors) approves specific detectors and experiments with the objective of having the best scientific program within the overall available resources. Initial estimates for detectors for some of the smaller construction projects were not done to the same level of detail as with larger projects. This is not the case for the SSC where a possible, though not optimized, scenario was laid out and costed. An additional reason for not including R&D and detector costs in the table is that for the earlier projects, these costs were not carefully segregated and identified, in part for the reasons just described.

Table B shows the initial estimated costs (EC) and actual completed costs (CC) for each project in then-year dollars. These costs were respectively escalated to FY 1988 dollars using the funding profile originally estimated and that actually spent. The ratio CC/EC shows that five out of seven of these construction projects came in within a few percent of the initial cost estimate. Two of the smaller projects had overruns for the reasons already discussed. The larger projects did come in at (or even under) cost. These large projects were carefully costed beforehand and were carefully designed using proven technology. Summing up the seven construction projects, the total initial estimated cost in FY 1988 dollars is $2,075 million and the total completed costs, $2,099 million, an overrun of 1.2 percent, a truly remarkable record in spite of the problems with the two venturesome projects requiring new technology.

While the CBO correctly quotes the initial estimates presented to the Congress for the Tevatron I project, the project was redefined to take advantage of improved technology and the Congress was informed of, and approved, the increased cost before substantial spending was committed to the project. The wisdom of this change, and of the Congress approving the redefined project, is now proven with the benefit of hindsight. The Tevatron is now, and will be for several years, the highest energy accelerator in the world. It is operating at record-breaking event rates and is the flagship for U.S. high energy physics. The CBO accounting, by contrast, merely shows this project as an embarrassing overrun.

SHARING THE COST: STATE AND FOREIGN CONTRIBUTIONS

Foreign and State contributions would lower the cost of the project to the Federal Government. While the details are not yet settled, there is reason to anticipate that contributions from the host State will be substantial.

The Department is actively pursuing prospects for international participation and cost sharing in the SSC. Based on initial responses, the Department believes that substantial contributions are likely. Considerable interest has been shown in the superconducting magnets. In addition, interest has been expressed in a number of other components and systems such as the injector, radio frequency, and control systems. Individual foreign scientists and engineers are already participating in design work, and this is likely to increase considerably as the project moves forward. Thus, in-kind contributions to the facility are expected in a number of areas. It is clear that there will be substantial foreign contributions to the SSC detectors. Understandably, scientists around the world are eager to participate in building these detectors and using them for experiments. Contributions to the detectors will build on a long and highly successful history of international collaboration on high energy physics detectors.

While high energy physics funds in Europe are presently largely committed, some resources will become available for SSC collaboration as individuals and institutions redirect their work to new possibilities. In particular, following the completion of ongoing construction projects, resources should become available. In addition, many of the in-kind contributions to the accelerator facilities are expected to come from sources other than those which traditionally fund high energy physics. Contributions on the order of 20 percent are considered to be a realistic expectation.

Chapter IV
Congressional Options

The CBO report explains that the Congress could decide to build the SSC, defer the decision, join CERN in building the LHC, or build an electron-positron collider. Table I in the summary of the CBO report compares the alternative accelerator "options" and the risks and benefits associated with these options.

BUILD THE SSC

Few in the scientific community will disagree about the fundamental nature of particle physics and the traditional leading role that the U.S. has played in this field. The SSC was recommended unanimously by the High Energy Physics Advisory Panel to the DOE as top priority for the U.S. high energy physics program over 5 years ago. Since that time, this view has been reiterated on a number of occasions by both HEPAP and a National Academy of Sciences (NAS) committee which studied all of physics. There are no technical alternatives, and the field cannot maintain its strength unless the SSC is built.

Timely construction of the SSC will be an outstanding investment in America's future. It will give the U.S. an unmatched scientific instrument capable of producing scientific results of immense significance in a realm which cannot be explored by any other facility, existing or planned. Beyond its primary scientific purpose, the SSC will have direct benefits to U.S. industry in terms of advanced technology spin-off which will contribute materially to the technological competitiveness and economic health of the nation. As in the past, we anticipate that benefits from technologies developed for the SSC will include advances in medical treatment and diagnosis; breakthroughs in numerous industrial processes, data processing, and electronics. The SSC, with its scientific challenge and promise of profound discoveries, will attract many of the best young minds into science and engineering thereby ensuring an adequate supply of scientific and engineering talent to serve future generations. A bold, exciting venture such as the SSC is required to ensure continued U.S. excellence in basic science and technological advancement which are the underpinning for economic competitiveness. The SSC is a historic opportunity for our nation. We have assembled a team of expert, enthusiastic personnel, and garnered the support of scientists, educators, and industrialists, in order to proceed with building the SSC. The scientific, technological, educational, and economic benefits associated with such a premier facility can only be reaped by the United States moving ahead aggressively to make the SSC a reality.

DEFER THE DECISION

The CBO report does not adequately address the costs of deferring construction. Noticeably absent is any discussion on the scientific, educational, technological, and economic benefits to the nation that would be lost if the SSC is delayed. Contrary to the CBO report, SLAC and Tevatron I do not have enough scientific capacity to occupy U.S. physicists until the late 1990's. These facilities will be replaced at the frontier by the new projects presently under construction in Europe and in the Soviet Union.

Of course, significant delays would also cause cost escalation; spreading the work over more years increases the personnel costs, the various categories of overhead and support, and the impact of inflation. More important, is the risk of breaking up the established support community: scientists, engineers, architects, manufacturers, and suppliers. Without a clear signal to move ahead with the SSC, much of this team will become committed to other projects and investments. Bringing back this expertise after a hiatus would prove costly in terms of time and money.

The chances for substantial international contributions to the SSC facility would also be put at risk by a significant delay. Foreign countries would become committed to more timely projects elsewhere and be unable to participate vigorously in the SSC.

JOIN CERN IN BUILDING THE LHC

This option would be an unwise investment for America. The LHC would not be as powerful as the SSC and would not be able to advance the science frontier as far. Even if the LHC alternative did not have serious logistical, technical, and scientific difficulties, it would still not provide an adequate alternative for the U.S. high energy physics community. It would represent a drastic cutback for the total opportunities worldwide, quite apart from the national interest related to having a strong U.S. program. The present interchanges and collaborative efforts between the U.S. and Europe, in the field of high energy physics, can work because both regions are equal partners with facilities of their own. This arrangement would no longer work were the U.S. to abandon the idea of U.S.-based accelerators.

In addition, the U.S. would lose all the advantages that accrue from building the accelerator on its own soil and doing physics "at home" rather than abroad. The national pride and prestige and encouragement to young people to enter scientific fields that come from pursuing the frontier in this fundamental field would be lost. Training opportunities for scientists, engineers, and technicians in many advanced technologies would be gone. The stimulation of industries in many high technology areas would no longer be there. Some of our best minds would have to go abroad to pursue their interests in fundamental science. The project of putting a man on the moon undoubtedly contributed a great deal to our scientific and technological strength; it is doubtful that this could have been achieved if we had just participated in someone else's effort.

BUILD AN ELECTRON-POSITRON COLLIDER

As discussed in the summary, this project has not yet been proposed by the scientific community and a demonstration of its feasibility is years away. It should be noted that to arrive at its "Estimated Cost" for the electron-positron collider the CBO report assumes technical parameters considerably beyond those presently obtained in practical devices. Many other advances will also be needed. In particular, the required beam quality will have to be improved dramatically as the energy is increased. In addition, the extreme forces of one beam on the other must be understood. Electron-positron and proton-proton colliders are complementary in that some phenomena are better studied with one and some with the other. High energy electron-positron colliders will be needed, but the technology is not presently available. A conceptual design for even an intermediate energy electron-positron linear collider cannot start until at least 1992, and in the meantime any cost estimates are at best unreliable guesses.

CONCLUSION

The Department stands by its decision to build the SSC and by its $4.4 billion (1988 dollars) cost estimate. The lessons learned in successfully building high energy physics facilities like Fermilab and SLAC, and in working through technologically challenging projects like the Tevatron I and Energy Saver, enable the Department to propose the SSC with confidence.

The SSC is more than a necessary scientific instrument; it is an investment in America's future. As Leon Lederman, the recent Nobel Laureate in Physics, has said, "The doing of science, as well as the supporting of science, is an expression of faith in the future. It would have been possible to have told Newton and Faraday, Maxwell and Einstein that given the poverty and squalor around them, their research endeavors were unaffordable luxuries. To have done so should have prevented the science that led to the economic gains that are the major factor in relieving the poverty and squalor. Without support for research like the Super Collider, America is in danger of losing its competitive edge, its traditional control of high-tech trade. Not only will we have to learn from Japan and Western Europe but from South Korea and Singapore as well. I hope that we are not on the verge of a political decision that will save a few dollars and lose untold benefits."

Table 8

HEP Accelerator Projects Completed Since 1965

Site	Facility	Const. Start	On Schedule for Beam Turn-on	Initial Est. Cost (EC) (Then-Year $)	Completed Cost (CC) (Then-Year $)	Initial Est. Cost (EC) (FY88$)	Completed Cost (CC) (FY88$)	CC/EC	Remarks
SLAC	Linac & Lab	1962	Yes	$ 114M	$ 114M	$ 630	$ 630	1.00	
FNAL	Original Lab	1969	Ahead of schedule by one year	250M	243M	968	921	0.95	Design energy of 200 GeV; increased to 400 GeV.
SLAC	PEP	1976	Yes	78M	78M	160	160	1.00	
FNAL	Energy Saver	1979	No-delay of about 2 years	39M	51M	67	84	1.25	Problems mastering superconducting magnets.
FNAL	Tevatron I	1981	No-delay of about 3 years	39.5M	84M	55	106	1.93	Design changed from electron cooling of antiprotons to stochastic cooling to ensure greater probability of meeting technical goals. Design change caused both cost increase and schedule delay.
FNAL	Tevatron II	1982	Yes	49M	49M	62	62	1.00	
SLAC	SLC	1984	Yes	$ 112M	$ 115M	$ 133	$ 136	1.02	Cost increase because of decrease in rate of early funding.
						$2,075	$2,099	1.01	

APPENDIX I

READINESS OF THE SUPERCONDUCTING SUPER COLLIDER MAGNETS

Much has been accomplished by the past five years of research & development (R&D) work and the basic magnet design is now in hand. The advances in magnet technology assumed for the SSC design have been demonstrated--design field, field alignment and uniformity, refrigeration requirements, and magnet protection. Engineering refinements will continue in the laboratories and as industry further develops and implements the manufacturing methods to efficiently produce the nearly 10,000 magnets.

Many of the features of the SSC magnet design were first proven on a series of about 30 short magnets, mostly 3 to 6 feet in length, but with six of them having a length of 15 feet. These magnets showed that the high quality field needed (uniform at the 0.01 percent level) could be achieved and gave valuable experience with coil winding and clamping techniques.

The short magnets showed that the coil clamping techniques used are inherently robust and have the margin to withstand forces well beyond those specified. In particular, several of these magnets were operated at over 9 tesla (made possible by reducing the temperature so that the superconductor could carry even more current). Since the forces go as the square of the magnetic field, at 9 tesla, the forces are about twice those at the specified 6.6-tesla field.

The first full-length (55 feet) magnets did not train well and had trouble getting to the nominal field. The cause turned out to be the mechanical interface between the coil package and the magnet yoke, a problem that had not shown up in the shorter magnets. Two solutions were proposed and both have recently been demonstrated to work.

To date, nine full-length magnets have been tested. Two recent magnets quickly achieved the nominal SSC field. One of the earlier magnets also achieved this field, but only after energizing the magnet several times. All nine of these magnets have been successful from the R&D point of view: they have furthered our understanding of the SSC magnets and how best to build them. For example, even the first long magnets gave valuable information of field uniformity, verified the mounting in cryostats, and showed that the magnets could be safely quenched without overheating.

Two long magnets have been tested to failure. In each case, an electrical short developed and the resulting arc melted some nearby metal, much as a short in the plug of a household appliance can cause damage. These arcs opened a passage to the bore tube through which the liquid helium could escape and quickly vaporize in a dramatic fashion. There was no externally observable damage to the magnets--the damage was all contained internally. Autopsies of these magnets have been highly informative.

While the R&D program is proceeding toward its goal of optimizing the operating margin and reliability of the magnets for a cost-effective magnet design, industry is being given detailed information on these magnets through a technology orientation program, the first of three phases of the Magnet Industrialization Program. In January and February 1989, industrial engineers will visit the three national laboratories doing SSC magnet R&D. The second phase will begin later in 1989 with the design and fabrication of tooling by industry, leading to industrial prototype magnets and eventual production. Industry has already made important contributions to improvements in the SSC superconducting cable, and most of the components for the test magnets and their cryostats are already being purchased from industry.

To conclude, the advances in magnet technology assumed for the SSC design are now in hand--design field, field alignment, field uniformity, refrigeration required, and a safe temperature rise in those instances when the magnets lose superconductivity -- all have been demonstrated in the long magnets that have been built and tested. The final engineering and manufacturing details will be worked out over the next year or two with the schedule calling for full industrial production of the magnets beginning in 1993, as required for the planned SSC project with its construction start in FY 1990.

United States General Accounting Office

GAO

Report to Congressional Requesters

January 1989

FEDERAL RESEARCH

Determination of the Best Qualified Sites for DOE's Super Collider

GAO/RCED-89-18

APPENDIX II

Additional Material Submitted for the Record

Council for
Chemical
Research, Inc.

One Bethlehem Plaza, Suite 911
Bethlehem PA 18018
February 17, 1989

The Honorable Wendell H. Ford, Chairman
Subcommittee on Energy Research and Development
Committee on Energy and Natural Resources
United States Senate
Washington, DC 20510

Dear Chairman Ford:

The Council for Chemical Research herewith offers some observations about the role of the research supported by the Department of Energy's Basic Energy Sciences (BES) program.

The Council for Chemical Research, formed in 1979, works hard to strengthen the U.S. economy, competitiveness and quality of life.

We do this in several ways, but chiefly by a dual combination: improving interactions between academia and industry, and improving support for basic research in chemistry and chemical engineering. Our members represent the 157 universities where nearly all the U.S. academic research in these fields is done, and the 55 major companies which carry out most of the corresponding industrial research.

All the data we can find show that our companies contribute a much larger share of the expenditures of pertinent university departments than do any other industries to their pertinent departments.

Necessarily, however, the major source of support for this nation's scientific research in universities is the Federal Government. This combination of public and private support is uniquely well repaid by chemistry and chemical engineering, which produce a net positive trade balance of over $10 billion annually. That is an outstanding and, we believe, unique trade balance for a technological discipline. It ties closely to the phrase, "the central science," often applied to chemistry and chemical technology.

We present two reasons, each vital to the nation's security and economy, why the Council finds the BES program to be exceptionally valuable and worthy of allowance of the full FY 1990 request. One reason has to do with the need for new energy-related knowledge; the other with the needs for people who can generate that knowledge and for people who can use it.

Typifying the spectrum of chemistry's benefits to our society are: energy (over 90% is based on chemical processes), all food production, most medical advances (including the promise of biotechnology), and materials (including the promise of superconductors).

Two National Academy of Sciences reports describe very well the accomplishments and exciting expectations from research in chemistry and chemical engineering. They are "Frontiers in Chemical Engineering" (1988) and "Opportunities in Chemistry" (1985). Attached is a short summary of each.

We hope that the foregoing background will help the Subcommittee appreciate the Council's two reasons for stating that the nation's security and economy down the road will be significantly affected by whether or not the Congress allows the full BES request.

The first reason is the urgent need for advanced knowledge pertinent to energy. World petroleum production will peak out in the vicinity of the year 2000. U.S. reserves and production have already declined significantly despite much effort. It will take more than a few years to develop substitute technologies.

Clearly, new fuels and new methods of conservation will soon be needed. For either of these, basic chemical research for new chemical knowledge is absolutely vital. Chemistry is fundamental to the whole sequence of obtaining new liquid fuels, whether from coal, oil shale, natural gas or biomass. It is equally fundamental to obtaining economical new methods of conservation and to further improving the efficiency of combustion.

Brief examples might help make clear what we are saying:

(1) Some new fuels will come from sources, such as coals, that have lower energy content and higher sulfur content than those now used. Achieving acceptable prices for producing, cleaning, converting and burning these fuels, without environmental harm, will depend on better knowledge for control of chemical reactivity.

(2) The possibility of splitting water with sunlight to form hydrogen, the cleanest of all fuels, will depend entirely on new knowledge of photochemistry and catalysis.

(3) New uses of biomass fuels will need new biological and chemical knowledge as the bases for faster growth of trees and other plants, in various climates. New knowledge is also needed concerning the means of reacting these substances and separating and purifying the desired products for the

economical manufacture of fuels and chemicals from biological materials.

(4) If we find ourselves more dependent on nuclear-generated electricity, as seems likely, again basic chemical research will be required. That stems from needs for knowledge underlying new materials for smaller, safer nuclear generating plants, and new processes for preparing and post-treatment of their nuclear fuel.

(5) If we are to take seriously the greenhouse threat to world climate, we must cut back on carbon dioxide emissions from automotive engines and electric power plants. New knowledge will be a necessary part of this.

(6) Combustion, a chemical process, is still our energy mainstay, and it will be depended on for many decades to come. Improved energy conservation, a vital component of the health of our nation's economy, is an essential factor that relies, in no small part, on expanding our knowledge and understanding of combustion processes and raising their efficiency.

We come now to the second reason for the Council's strong concern that the full request for Basic Energy Sciences be allowed: trained people.

We read much about and are beginning to experience the severe shortfall in numbers of bright young people who are capable of, and opting for, careers in mathematics, science and engineering.

Coming needs for well prepared people, in the laboratories and plants which must plan to rescue the U.S. from severe shortages of energy, number far beyond the count of young people who will be emerging from the pipeline when needed. A disaster threatens in energy, as in all fields of science and technology.

The BES research program, in universities and in many projects at national laboratories, depends on faculty-guided efforts of graduate students and postdoctoral people in the very areas of science and engineering which will be crucial to economic and environmentally acceptable energy supply and conservation.

Therefore, as an essential part of the pipeline of well-prepared people in exactly those disciplines needed for continued energy viability, the BES program should be encouraged in every way possible, including providing for the full (though still constrained) BES request for FY 1990.

SUMMARY

The Council for Chemical Research, for two reasons, urges that the Congress allow the full FY 1990 budget request of the Depart-ment of Energy's Basic Energy Sciences program. The $590 million request is 2.5% above the FY 1989 figure in real dollars, but it still falls short of the acutely growing need for new energy knowledge and for building, equipping and operating DOE's unique facilities for use of the U.S. scientific and engineering communities. The Council's two reasons for strong support of the BES request are (1) the bleak energy future now faced by the U.S., unless we move to add abundantly to our knowledge and put it to use, and (2) the vital training role of BES research, to help counter the severe shortfall of well-prepared people in science and engineering, especially in energy fields.

Chairman Ford and Members of the Subcommittee, we thank you for your attention to our views. We hope that we have been helpful on a most important matter.

Respectfully yours,

Judson King

C. Judson King
Chairman

FRONTIERS IN CHEMICAL ENGINEERING

RESEARCH NEEDS AND OPPORTUNITIES

Committee on Chemical Engineering Frontiers:
Research Needs and Opportunities
Board on Chemical Sciences and Technology
Commission on Physical Sciences, Mathematics, and Resources
National Research Council

NATIONAL ACADEMY PRESS
Washington, D.C. 1988

NATIONAL ACADEMY PRESS • 2101 Constitution Avenue, NW • Washington, DC 20418

NOTICE: The project that is the subject of this report was approved by the Governing Board of the National Research Council, whose members are drawn from the councils of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine. The members of the committee responsible for the report were chosen for their special competences and with regard for appropriate balance.

This report has been reviewed by a group other than the authors according to procedures approved by a Report Review Committee consisting of members of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine.

The National Academy of Sciences is a private, nonprofit, self-perpetuating society of distinguished scholars engaged in scientific and engineering research, dedicated to the furtherance of science and technology and to their use for the general welfare. Upon the authority of the charter granted to it by the Congress in 1863, the Academy has a mandate that requires it to advise the federal government on scientific and technical matters. Dr. Frank Press is president of the National Academy of Sciences.

The National Academy of Engineering was established in 1964, under the charter of the National Academy of Sciences, as a parallel organization of outstanding engineers. It is autonomous in its administration and in the selection of its members, sharing with the National Academy of Sciences the responsibility for advising the federal government. The National Academy of Engineering also sponsors engineering programs aimed at meeting national needs, encourages education and research, and recognizes the superior achievements of engineers. Dr. Robert M. White is president of the National Academy of Engineering.

The Institute of Medicine was established in 1970 by the National Academy of Sciences to secure the services of eminent members of appropriate professions in the examination of policy matters pertaining to the health of the public. The Institute acts under the responsibility given to the National Academy of Sciences by its congressional charter to be an adviser to the federal government and, upon its own initiative, to identify issues of medical care, research, and education. Dr. Samuel O. Thier is president of the Institute of Medicine.

The National Research Council was organized by the National Academy of Sciences in 1916 to associate the broad community of science and technology with the Academy's purposes of furthering knowledge and advising the federal government. Functioning in accordance with general policies determined by the Academy, the Council has become the principal operating agency of both the National Academy of Sciences and the National Academy of Engineering in providing services to the government, the public, and the scientific and engineering communities. The Council is administered jointly by both Academies and the Institute of Medicine. Dr. Frank Press and Dr. Robert M. White are chairman and vice-chairman, respectively, of the National Research Council.

Support for this project was provided by the American Chemical Society, the American Institute of Chemical Engineers, the Council for Chemical Research, Inc., the U.S. Department of Energy under Grant No. DE-FG01-85FE60847, the National Bureau of Standards under Grant No. 50SBNB5C23, the National Science Foundation under Grant No. CBT-8419184, and the Whitaker Foundation.

Library of Congress Cataloging-in-Publication Data

National Research Council (U.S.). Committee on Chemical Engineering
Frontiers: Research Needs and Opportunities.
Frontiers in chemical engineering : research needs and
opportunities/Committee on Chemical Engineering Frontiers—
Research Needs and Opportunities, Board on Chemical Sciences and
Technology, Commission on Physical Sciences, Mathematics, and
Resources, National Research Council.
p. cm.
Bibliography: p.
Includes index.
ISBN 0-309-03793-X (paper); ISBN 0-309-03836-7 (cloth)
1. Chemical engineering—Research—United States. I. Title.
TP171.N37 1988 88-4120
620'.0072—dc19 CIP
(Rev.)

No part of this book may be reproduced by any mechanical, photographic, or electronic process, or in the form of a phonographic recording, nor may it be stored in a retrieval system, transmitted, or otherwise copied for public or private use without written permission from the publisher, except for the purposes of official use by the U.S. government.

Printed in the United States of America

Committee on Chemical Engineering Frontiers: Research Needs and Opportunities

---

NEAL R. AMUNDSON (*Chairman*), University of Houston
EDWARD A. MASON (*Vice-Chairman*), Amoco Corporation
JAMES WEI (*Vice-Chairman*), Massachusetts Institute of Technology
MICHAEL L. BARRY, Vitelic Corporation
ALEXIS T. BELL, University of California, Berkeley
KENNETH B. BISCHOFF, University of Delaware
HERBERT D. DOAN, Doan Associates
ELISABETH M. DRAKE, Arthur D. Little, Inc.
SERGE GRATCH, Ford Motor Company (retired)
HUGH D. GUTHRIE, Morgantown Energy Technology Center, DOE
ARTHUR E. HUMPHREY, Lehigh University
SHELDON E. ISAKOFF, E.I. du Pont de Nemours and Company, Inc.
JAMES LAGO, Merck and Company (retired)
KEITH W. MCHENRY, JR., Amoco Oil Company
SEYMOUR L. MEISEL, Mobil Research and Development Company (retired)
ARTHUR B. METZNER, University of Delaware
ALAN S. MICHAELS, North Carolina State University
JOHN P. MULRONEY, Rohm and Haas Company
LEIGH E. NELSON, Minnesota Mining and Manufacturing Co., Inc.
JOHN A. QUINN, University of Pennsylvania
KENNETH J. RICHARDS, Kerr-McGee Corporation
JOHN P. SACHS, Horsehead Industries, Inc.
ADEL F. SAROFIM, Massachusetts Institute of Technology
ROBERT S. SCHECHTER, University of Texas, Austin
WILLIAM R. SCHOWALTER, Princeton University
L. E. SCRIVEN, University of Minnesota
JOHN H. SEINFELD, California Institute of Technology
JOHN H. SINFELT, Exxon Research and Engineering Company
LARRY F. THOMPSON, AT&T Bell Laboratories
KLAUS D. TIMMERHAUS, University of Colorado
ALFRED E. WECHSLER, Arthur D. Little, Inc.
ARTHUR W. WESTERBERG, Carnegie-Mellon University

ROBERT M. SIMON, *Project Director*
ROBERT M. JOYCE, *Editorial Consultant*
NANCY WINCHESTER, *Editor*
ROSEANNE PRICE, *Editor*
LYNN E. DUFF, *Financial Assistant*
MONALISA R. BRUCE, *Administrative Secretary*

Panels of the Committee

Panel on Biochemical and Biomedical Engineering

ARTHUR E. HUMPHREY (*Chairman*), Lehigh University
KENNETH B. BISCHOFF, University of Delaware
CHARLES BOTTOMLEY, E.I. du Pont de Nemours and Company, Inc.
STUART E. BUILDER, Genentech, Inc.
ROBERT L. DEDRICK, National Institutes of Health
MITCHELL LITT, University of Pennsylvania
ALAN S. MICHAELS, North Carolina State University
FRED PALENSKY, Minnesota Mining and Manufacturing Co., Inc.

Panel on Electronic, Photonic, and Recording Materials and Devices

LARRY F. THOMPSON (*Chairman*), AT&T Bell Laboratories
LEE F. BLYLER, AT&T Bell Laboratories
JAMES ECONOMY, IBM Almaden Research Center
DENNIS W. HESS, University of California, Berkeley
RICHARD POLLARD, University of Houston
T. W. RUSSELL FRASER, University of Delaware
MICHAEL SHEPTAK, Ampex Corporation

Panel on Advanced Materials

ARTHUR B. METZNER (*Chairman*), University of Delaware
FRANK BATES, AT&T Bell Laboratories
C. F. CHANG, Union Carbide Corporation
F. NEIL COGSWELL, Imperial Chemical Industries
WILLIAM W. GRAESSLEY, Princeton University
FRANK KELLEY, University of Akron
JOHN B. WACHTMAN, JR., Rutgers University
IOANNIS V. YANNAS, Massachusetts Institute of Technology

Panel on Energy and Natural Resources Processing

KEITH McHENRY (*Chairman*), Amoco Oil Company
LESLIE BURRIS, Argonne National Laboratory
ELTON J. CAIRNS, Lawrence Berkeley Laboratory
NOEL JARRETT, Alcoa Laboratories
FREDERIC LEDER, Dowell Schlumberger
JOHN SHINN, Chevron Research Company
REUEL SHINNAR, City College of New York
PAUL B. WEISZ, University of Pennsylvania

Panel on Environmental Protection, Safety, and Hazardous Materials

ADEL SAROFIM (*Chairman*), Massachusetts Institute of Technology
SIMON L. GOREN, University of California, Berkeley
GREGORY J. MACRAE, Carnegie Mellon University
THOMAS W. PETERSON, University of Arizona
WILLIAM RODGERS, Oak Ridge National Laboratory
GARY VEURINK, Dow Chemical Company
RAY WITTER, Monsanto Corporation

Panel on Computer Assisted Process and Control Engineering

ARTHUR W. WESTERBERG (*Chairman*), Carnegie Mellon University
HENRY CHIEN, Monsanto Corporation
JAMES M. DOUGLAS, University of Massachusetts
BRUCE A. FINLAYSON, University of Washington
ROLAND KEUNINGS, University of California, Berkeley
MANFRED MORARI, California Institute of Technology
JEFFREY J. SIIROLA, Eastman Kodak Company
WILLIAM SILLIMAN, Exxon Production Research Company

Panel on Surface and Interfacial Engineering

ALEXIS T. BELL (*Chairman*), University of California, Berkeley
RICHARD C. ALKIRE, University of Illinois
JOHN C. BERG, University of Washington
L. LOUIS HEGEDUS, W. R. Grace and Company
ROBERT JANSSON, Monsanto Corporation
KLAVS F. JENSEN, University of Minnesota
JAMES R. KATZER, Mobil Research and Development Company
LEIGH E. NELSON, Minnesota Mining and Manufacturing Company
LANNY D. SCHMIDT, University of Minnesota

Board on Chemical Sciences and Technology

EDWARD A. MASON (*Co-Chairman*), Amoco Corporation
GEORGE M. WHITESIDES (*Co-Chairman*), Harvard University
NEAL R. AMUNDSON, University of Houston
JOHN I. BRAUMAN, Stanford University
GARY FELSENFELD, National Institutes of Health
WILLIAM A. GODDARD III, California Institute of Technology
JEANETTE G. GRASSELLI, BP America
MICHAEL L. GROSS, University of Nebraska
RALPH HIRSCHMANN, University of Pennsylvania
ROBERT L. LETSINGER, Northwestern University
JAMES F. MATHIS, Exxon Chemical Company
GEORGE C. PIMENTEL, University of California, Berkeley
JOHN A. QUINN, University of Pennsylvania
STUART A. RICE, University of Chicago
FREDERIC M. RICHARDS, Yale University
ROGER A. SCHMITZ, University of Notre Dame
L. E. SCRIVEN, University of Minnesota
DAVID P. SHEETZ, Dow Chemical USA
LEO J. THOMAS, JR., Eastman Kodak Company
NICHOLAS J. TURRO, Columbia University
MARK S. WRIGHTON, Massachusetts Institute of Technology

ROBERT M. SIMON, *Staff Director*
WILLIAM SPINDEL, *Special Staff Adviser*
PEGGY J. POSEY, *Staff Associate*
LYNN E. DUFF, *Financial Assistant*

Commission on Physical Sciences, Mathematics, and Resources

NORMAN HACKERMAN (*Chairman*), Robert A. Welch Foundation
GEORGE F. CARRIER, Harvard University
DEAN E. EASTMAN, IBM Corporation
MARYE ANN FOX, University of Texas, Austin
GERHART FRIEDLANDER, Brookhaven National Laboratory
LAWRENCE W. FUNKHOUSER, Chevron Corporation (retired)
PHILLIP A. GRIFFITHS, Duke University
J. ROSS MacDONALD, The University of North Carolina at Chapel Hill
CHARLES J. MANKIN, The University of Oklahoma
PERRY L. McCARTY, Stanford University
JACK E. OLIVER, Cornell University
JEREMIAH P. OSTRIKER, Princeton University Observatory
WILLIAM D. PHILLIPS, Washington University
DENIS J. PRAGER, MacArthur Foundation
DAVID M. RAUP, University of Chicago
RICHARD J. REED, University of Washington
ROBERT E. SIEVERS, University of Colorado
LARRY L. SMARR, University of Illinois
EDWARD C. STONE, JR., California Institute of Technology
KARL K. TUREKIAN, Yale University
GEORGE W. WETHERILL, Carnegie Institution of Washington
IRVING WLADAWSKY-BERGER, IBM Corporation

RAPHAEL G. KASPER, *Executive Director*
LAWRENCE E. McCRAY, *Associate Executive Director*

Contents of the Full Report

ONE Executive Summary / 1

TWO What Is Chemical Engineering? / 9

THREE Biotechnology and Biomedicine / 17

FOUR Electronic, Photonic, and Recording Materials and Devices / 37

FIVE Polymers, Ceramics, and Composites / 61

SIX Processing of Energy and Natural Resources / 79

SEVEN Environmental Protection, Process Safety, and Hazardous Waste Management / 105

EIGHT Computer-Assisted Process and Control Engineering / 135

NINE Surfaces, Interfaces, and Microstructures / 153

TEN Recommendations / 175

APPENDIXES

A Detailed Recommendations for Funding / 185

B Contributors / 198

C The Chemical Processing Industries / 201

Index / 205

Contents

Executive Summary / 1
Recommendations / 11

FRONTIERS IN CHEMICAL ENGINEERING

RESEARCH NEEDS AND OPPORTUNITIES

Executive Summary

[illegible]HEMICAL ENGINEERING occupies a special place among scientific and engineering disciplines. It is an engineering discipline with deep roots in the world of atoms, molecules, and molecular transformations. The principles and approaches that make up chemical engineering have a long and rich history of contributions to the nation's technological needs. Chemical engineers play a key role in industries as varied as petroleum, food, artificial fibers, petrochemicals, plastics, ceramics, primary metals, glass, and specialty chemicals. All these depend on chemical engineers to tailor manufacturing technology to the requirements of their products and to integrate product design with process design. Chemical engineering was the first engineering profession to recognize the integral relationship between design and manufacture, and this recognition has been one of the major reasons for its success.

This report demonstrates that chemical engineering research will continue to address the technological problems most important to the nation. In the chapters that focus on these problems, many of the discipline's core research areas (e.g., reaction engineering, separations, process design, and control) will appear again and again. The committee hopes that by discussing research frontiers in the context of applications, it will illustrate both the intellectual excitement and the practical importance of chemical engineering.

The research frontiers discussed in this report can be grouped under four overlapping themes: starting new technologies, maintaining leadership in established technologies, protecting and improving the environment, and developing systematic knowledge and generic tools. These frontiers are described in detail in Chapters 3 through 9. From among these, the committee has selected eight high-priority topics that merit the attention of researchers, decision makers in academia and industry, and organizations that fund or otherwise support chemical engineering. These high-priority areas are described below. Recommendations from the committee for initiatives that would permit chemical engineers to exploit these areas are briefly stated in Chapter 10 and detailed in Appendix A.

RESEARCH FRONTIERS IN CHEMICAL ENGINEERING

Starting New Technologies

Chemical engineers have an important role to play in bringing new technologies to commercial fruition. These technologies have their origin in scientific discoveries on the atomic and molecular level. Chemical engineers understand the molecular world and are skilled in integrating product design with process design, process control, and optimization. Their skills are needed to develop genetic engineering (biotechnology) as a manufacturing tool and to create new biomedical devices, and to design new products and manufacturing processes for advanced materials and devices for information storage and handling. In the fierce competition for world markets in these technologies, U.S. leadership in chemical engineering is a strong asset.

Biotechnology and Biomedicine (Chapter 3)

The United States occupies the preeminent scientific position in the "new" biology. If America is to derive the maximum benefit of its investment in basic biological research—whether in the form of better health, improved agriculture, a cleaner environment, or more efficient production of chemicals—it must also assume a preeminent position in biochemical and biomedical engineering. This can be accomplished by carrying out generic research in the following areas:

- Developing chemical engineering models for fundamental biological interactions.
- Studying phenomena at biological surfaces and interfaces that are important in the design of engineered systems.
- Advancing the field of process engineering. Important generic goals for research include the development of separation processes for complex and fragile bioproducts; the design of bioreactors for plant and mammalian tissue culture; and the development of detailed, continuous control of process parameters by rapid, accurate, and noninvasive sensors and instruments.
- Conducting engineering analyses of complex biological systems.

Interferons

Cancer now afflicts one out of four adults. One of the more promising therapies for certain kinds of cancers involves the use of interferon, a protein that occurs in minute quantities in the body where it is an essential part of the body's immune system. Interferon can be produced outside the body in cultures of transformed lymphoblastoid cells. A few years ago, it was possible to culture these human cells on scales up to a few hundred milliliters. Chemical engineers have now developed reactors for the aseptic culture of human cells on a scale 100,000 times larger, making it possible to produce human interferon in practical useful quantities.

The problems encountered in this scale-up illustrate the difficulties of engineering a biological process. It is possible to achieve a 20 percent yield of interferon from human cell culture. Yet, the first attempts at large-scale production gave yields of less than 5 percent. This was because a by-product of the cell culture was a protease—an enzyme that breaks down proteins such as interferon. Chemical engineers cooperated with biochemists in designing protease inhibitors and made a crucial contribution to the problem by designing a continuous, short-run process that minimized exposure of the interferon to the protease. Chemical engineers have also developed chromatographic separation systems that improve the yield by separating interferon from the reaction mixture quickly and efficiently.

As medical researchers continue to explore the therapeutic properties of interferons, chemical engineers will continue to provide the expertise needed to make available the quantities of these molecules necessary for clinical evaluation.

Electronic, Photonic, and Recording Materials and Devices (Chapter 4)

The character of American industry and society has changed dramatically over the past three decades as we have entered the "information age." New information technologies have been made possible by materials and devices whose structure and properties can be controlled with exquisite precision. This control is largely achieved by the use of chemical reactions during manufacturing. Future U.S. leadership in microelectronics, optical information technologies, magnetic data storage, and photovoltaics will depend on staying at the forefront of the chemical technology used in manufacturing processes. Chemical processing will also be a vital part of the likely manufacturing processes for high-temperature superconductors.

At the frontiers of chemical research in this area are a number of important challenges:

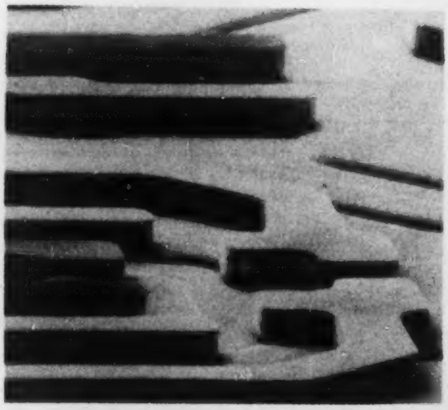
- Integrating individual chemical process steps used in the manufacture of electronic, photonic, and recording materials and devices. This is a key to boosting the yield, throughput, and reliability of overall manufacturing processes.

- Refining and applying chemical engineering principles to the design and control of the chemical reactors in which devices are fabricated.
- Pursuing research in separations applicable to the problem of ultrapurification. The materials used in device manufacture must be ultrapure, with levels of some impurities reduced to the parts-per-trillion level;
- Improving the chemical synthesis and processing of polymers and ceramics;
- Developing better processes for deposition and coating of thin films. An integrated circuit, in essence, is a series of electrically connected thin films. Thin films are the key structural feature of recording media and optical fibers, as well.
- Modeling the chemical reactions that are important to manufacturing processes and studying their dynamics.
- Emphasizing process design and control for environmental protection and process safety.

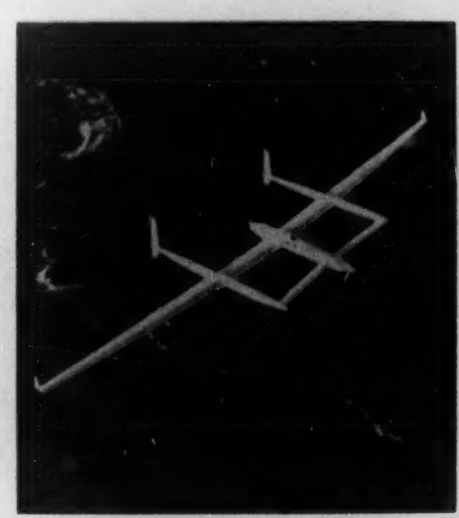
Microstructured Materials
(Chapters 5 and 9)

Advanced materials depend on carefully designed structures at the molecular and microscopic levels to achieve specific performance in use. These materials—polymers, ceramics, and composites—are reshaping our society and are contributing to an improved standard of living. The process technology used in manufacturing these materials is crucial—in many instances more important than the composition of the materials themselves. Chemical engineers can make important contributions to materials design and manufacturing by exploring the following research frontiers:

- Understanding how microstructures are formed in materials and learning how to control the processes involved in their formation.
- Combining materials synthesis and materials processing. These areas have traditionally been considered separate research areas. Future advances in materials require a fusion of these topics in research and practice.



Chemical reactions are used to achieve the fine structures seen in modern integrated circuits. This electron micrograph shows a transistor in a "cell" of a 1-megabit dynamic random access memory chip. The distance between features is about 1 μm. Courtesy, AT&T Bell Laboratories.



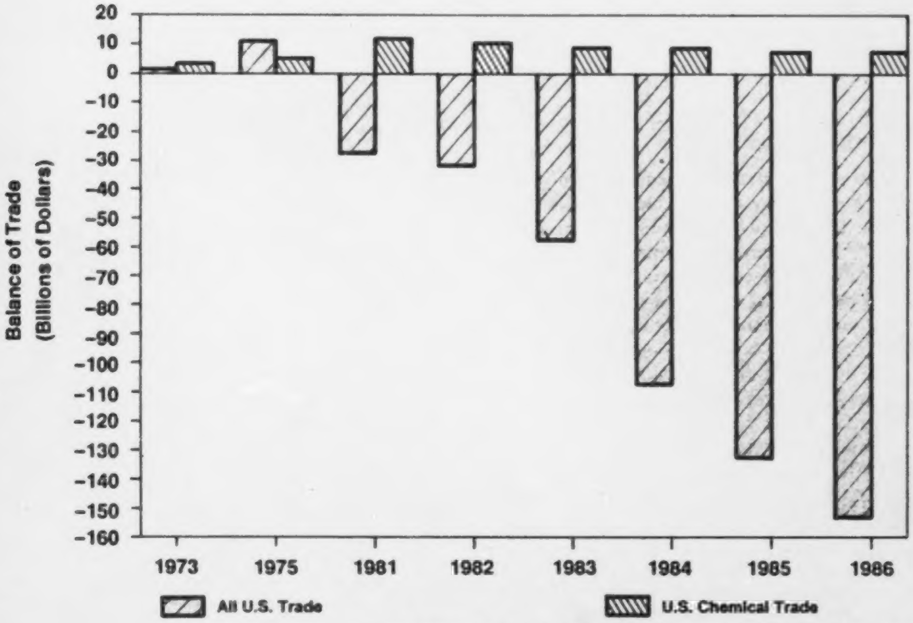
The first airplane to circle the globe without refueling was *Voyager*, which accomplished this feat in 1986. This novel aircraft was made possible by high-performance lightweight materials and adhesives that were used in its construction. Chemical engineering research is crucial to the design of such new materials and their large-scale, efficient production. Copyright 1986 by Doug Shane Visions.

- Fabricating and repairing complex materials systems. Mechanical methods currently in use (e.g., riveting of metals) cannot be applied reliably to the composite materials of the future. Chemical methods (e.g., adhesion and molecular self-assembly) will come to the fore.

Maintaining Leadership in Established Technologies

The U.S. chemical processing industries are one of the largest industrial sectors of the U.S. economy. The myriad of industries listed at the beginning of this chapter are pervasive and absolutely essential to society. The U.S. chemical industry is one of the most successful U.S. industries on world markets. At a time of record trade deficits, the chemical industry has maintained both a positive balance of trade and a growing share of world markets. The future international competitiveness of these industries should not be taken for granted. Far-sighted management in industry and continued support for basic research from both industry and government are required if this sector of the economy is to continue to contribute to the nation's prosperity.

In a report of this scope and size, it is not possible to spell out the research challenges faced by each part of the chemical processing industries. For example, the committee has reluctantly chosen to pass over food processing, a multibillion-dollar industry where chemical engineering finds a growing variety of applications. The committee has focused its discussion of challenges to the processing industries on energy and natural resources technologies. These



While the overall U.S. trade balance has plummeted to a deficit of more than $150 billion, the U.S. chemical industry has maintained a positive balance of trade. Courtesy, Department of Commerce.

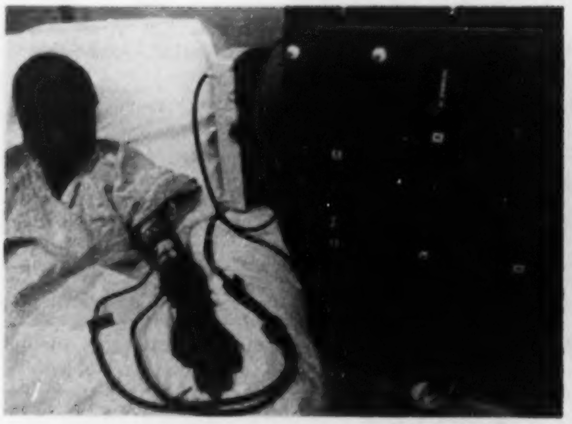


PLATE 1 The kidney dialysis machine (artificial kidney) is responsible for major reductions in deaths and adverse health consequences from kidney failure. Its development required a team effort that brought together chemical engineers, physicians, and materials scientists. The design of the disposable filter cartridge, shown attached to the front left side of the dialysis machine, was a major contribution by chemical engineers to the project. Courtesy, National Institute of Diabetes and Digestive and Kidney Diseases.

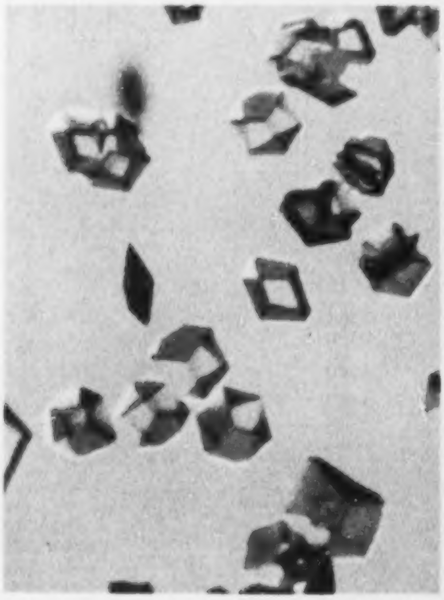


PLATE 2 These crystals of human insulin are made by bacteria whose genetic instructions have been altered using recombinant DNA techniques. Human insulin is needed by diabetics who develop allergies to the animal-derived insulin that has been used to treat the disease since 1921. Without chemical engineering contributions such as process design and purification technology, though, the large-scale production of human insulin would not be possible. Courtesy, Eli Lilly and Company.

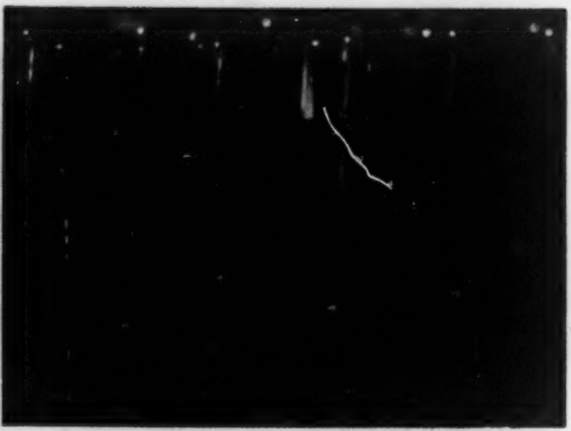


PLATE 3 Cell culture of plants now takes place in individual containers cared for by hand. Automation of plant cell culture using chemical engineering techniques could improve the yields and economics of plant cell culture and expand the range of its applications in the production of new species and hybrids. Courtesy, Monsanto Company.

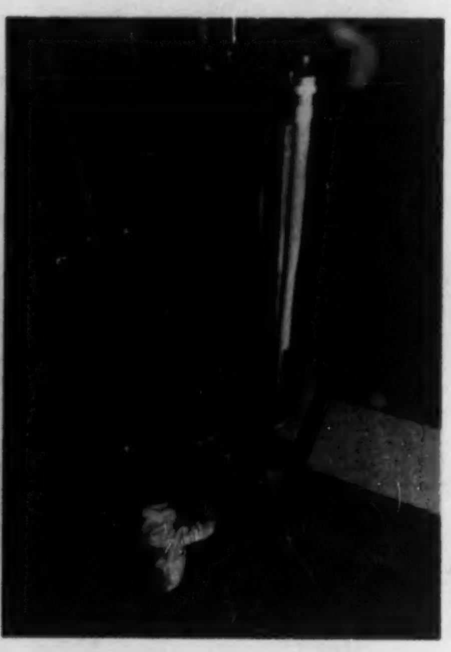
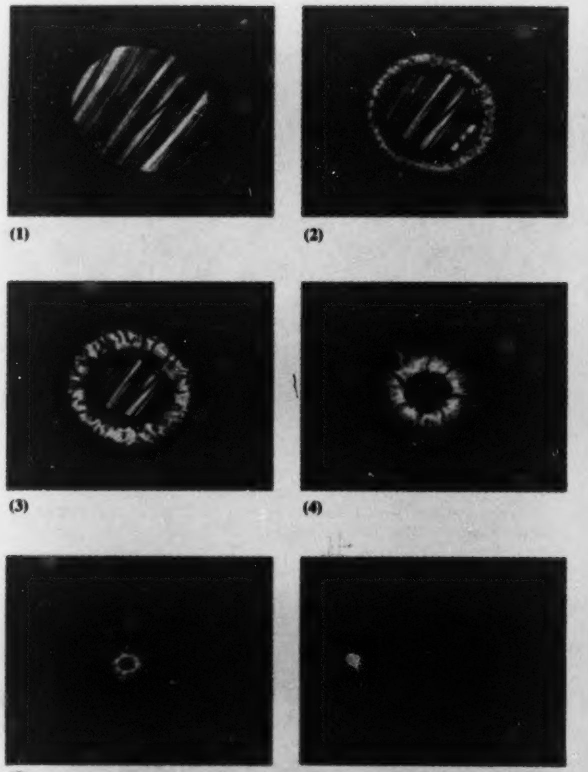


PLATE 4 Bioreactors that use mammalian cells, like this tower fermentor, are on the cutting edge of new biotechnology manufacturing processes. Courtesy, Cetus Corporation.



(1) (2) (3) (4) (5) (6)

PLATE 5 Stages in the retorting of an oil shale particle in a hot inert gas are shown. [1, 2, 3] The retorting zone moves inward as products diffuse out of the particle. A coke layer on the particle is formed as a final step of the retorting process. [4, 5, 6] Retorting goes to completion in the center of the particle, and a fully coked particle remains. Courtesy, Amoco Corporation.

PLATE 6 The turbulent environment in which combustion, a chemical process, takes place is dramatized by this Schlieren photograph of a propane diffusion flame. Courtesy, Norman A. Chigier, Carnegie Mellon University.

OZONE CONCENTRATIONS
16
LOW
HIGH

PLATE 7 Chemical engineers develop models to understand the formation, transport, and environmental fate of airborne pollutants such as ozone. This photograph shows a graphic display of a chemical engineering model for ozone concentrations in the Los Angeles basin. Courtesy, John Seinfeld, California Institute of Technology.

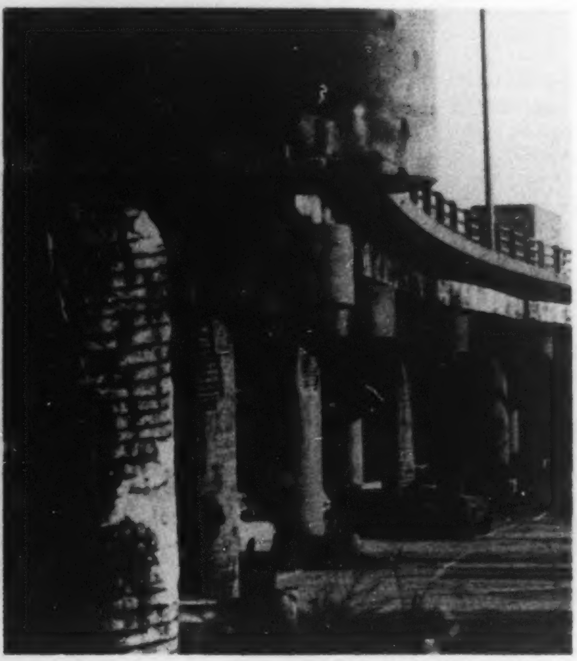
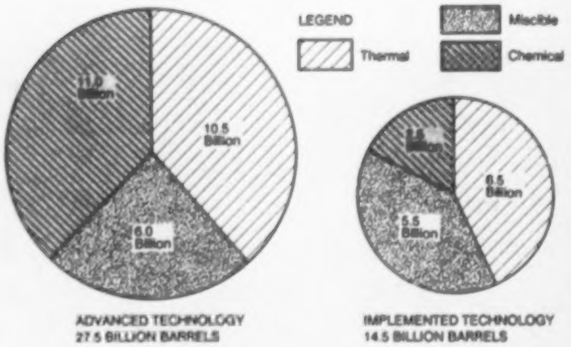


PLATE 8 Corrosion is a chemical process whose results are easy to see in the world around us. In this picture, corrosion of reinforcing steel has caused the concrete pillars to spall, weakening the bridge and forcing the installation of wooden joists to temporarily support the bridge deck structure. The results of corrosion impose significant economic costs on society—in 1982, these costs were estimated at about $120 billion. Courtesy, Robert Baboian, Texas Instruments, Inc.

technologies are key to supplying crucial national needs, keeping the United States competitive, and providing for national security. They are also the focus of substantial research and development in academia and government laboratories, in addition to industry. The committee has identified two high-priority initiatives to sustain the vitality and creativity of engineering research on energy and natural resources. These initiatives focus on in-situ processing of resources and on liquid fuels for the future.



Prospects for enhanced oil recovery using implemented and advanced technologies are shown above. The ultimate amount of U.S. oil that can be recovered by "implemented technology," technology that presently exists in at least the proven field test stage, is estimated to be 14.5 billion barrels. Using "advanced technology," technology that might be conceivably developed before 2013, adds another 13 billion barrels of oil to the estimate, for a total of 27.5 billion barrels. A comparison of the distribution of ultimate recoveries by method is also shown. Most of the increase in the estimate from applying advanced technology comes from improvements in chemical flooding methods. The projections assume that crude oil has a nominal price of $30 per barrel and that the minimum rate of return on capital is 10 percent. Reprinted with permission from *Enhanced Oil Recovery*. Copyright 1984 by the National Petroleum Council.

In-Situ Processing of Energy and Mineral Resources
(Chapter 6)

The United States has historically benefited from rich domestic resources of minerals and fuels located in readily accessible parts of the earth's crust. These easily reached resources are being rapidly depleted. Our remaining reserves, while considerable, require moving greater and greater amounts of the earth's crust to obtain and process resources, whether that crust is mixed with the desired material (as in a dilute ore vein) or whether it simply lies over the resource.

A long-range solution to this problem is to use chemical reactions to extract underground resources, with the earth itself as the reaction vessel. This is known as in-situ processing. Enhanced oil recovery is the most successful current example of in-situ processing, and yet an estimated 300 billion barrels of U.S. oil trapped underground in known reserves cannot be recovered with current technology. Long-range research aimed at oil, shale, tar sands, coal, and mineral resources is needed. Formidable problems exist both for chemists and for chemical engineers. Some research priorities for chemical engineers include separation processes, improved materials, combustion processes, and advanced methods of process design, scale-up, and control. Research on in-situ processing will require collaboration between chemical engineers and scientists and engineers skilled in areas such as geology, geophysics, hydrology, environmental science, mechanical engineering, physics, mineralogy, materials science, metallurgy, surface and colloid science, and chemistry.

Liquid Fuels for the Future
(Chapters 6 and 9)

Our current and foreseeable transportation technologies depend completely on a plentiful supply of liquid fossil fuels. Anticipatory research to ensure a future supply of these fuels is a wise investment. Research of this type subsumes a number of generic challenges in chemical engineering, including:

- Finding new chemical process pathways that can make large advances in the production of liquid fuels from solid and gaseous resources.

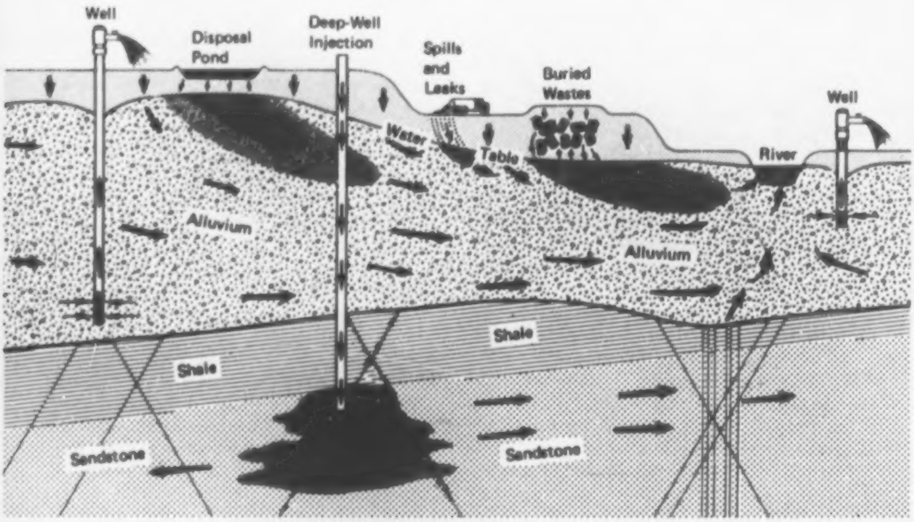
- Processing solids, since equipment design and scale-up are greatly limited by our lack of fundamental understanding of solids behavior.
- Developing better separation processes.
- Conducting research on materials capable of withstanding the extreme processing conditions that may be encountered when processing liquid fuels.
- Advancing the state of the art in the design, scale-up, and control of processes.

Protecting and Improving the Environment

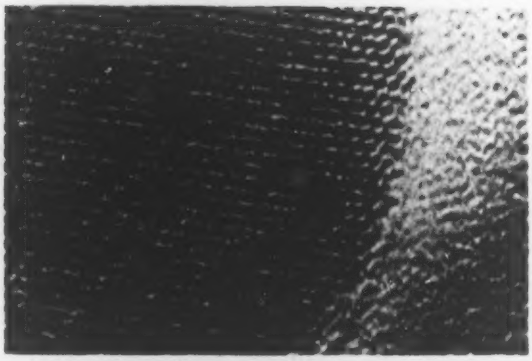
Responsible Management of Hazardous Substances (Chapter 7)

The modern world faces many environmental problems. Some of these are a consequence of producing the ever-increasing number and variety of chemicals and materials demanded by society. Chemical engineers must take up the role of cradle-to-grave guardians for chemicals, ensuring their safe and environmentally sound manufacture, use, and disposal. This means becoming involved in a range of research areas dealing with environmental protection, process safety, and hazardous waste management. In the following four areas, the challenges are clear, the opportunities for chemical engineering research are abundant, and the potential benefits to society are great.

- Conducting long-term research on the generation, control, movement, fate, detection, and environmental and health effects of contaminants in the air, water, and land. Chemical engineering research should include the fundamental investigation of combustion processes, the application of biotechnology to waste degradation, the development of sensors and measurement techniques, and participation in interdisciplinary studies of the environment's capacity to assimilate the broad range of chemicals that are hazardous to humans and ecosystems.
- Developing new chemical engineering design tools to deal with the multiple objectives of minimum cost; process resilience to changes



Past methods of waste disposal threaten water supplies today. Reprinted from *Opportunities in Chemistry*, National Academy Press, 1985.



This high-resolution electron micrograph shows the unique pore structure of the ZSM-5 zeolite catalyst. Molecules such as methanol and hydrocarbons can be catalytically converted within the pores to valuable fuels and lubricant products. Courtesy, Mobil Research and Development Corporation.

in inputs; minimization of toxic intermediates and products; and safe response to upset conditions, start-up, and shutdown.

- Directing research at cost-effective management of hazardous waste, as well as improved technologies (e.g., combustion) or new technologies for destroying hazardous waste.
- Carrying out research to facilitate multimedia, multispecies approaches to waste management. Acid rain and the leaching of hazardous chemicals from landfills demonstrate the mobility of chemicals from one medium (e.g., air, water, or soil) to another.

Developing Systematic Knowledge and Generic Tools

The success of chemical engineers in contributing to a diverse set of technologies is due to an emphasis on discovering and developing basic principles that transcend individual technologies. If, 20 years from now, chemical engineers are to have the same opportunities for contributing to important societal problems that they have today, then the research areas described in the preceding sections must be explored and supported in a way that maximizes the development of basic knowledge and tools.

In surveying the field of chemical engineering, the committee has identified two cross-cutting areas that are in a state of rapid development and that promise major contributions to a wide range of technologies. Accordingly, this report singles out for special attention the advances under way in applying modern computational methods and process control to chemical engineering and the promise of basic research in surface and interfacial engineering.

Advanced Computational Methods and Process Control (Chapter 8)

The speed and capability of the modern computer are revolutionizing the practice of chemical engineering. Advances in speed and memory size and improvements in complex problem-solving ability are more than doubling the effective speed of the computer each year. This unrelenting pace of advance has reached the stage where it profoundly alters the way in which chemical engineers can conceptualize problems and approach solutions. For example:

- It is now realistic to imagine mathematical models of fundamental phenomena beginning to replace laboratory and field experiments. Such computations increasingly allow chemical engineers to bypass the long (2 to 3 years), costly step of producing process and product prototypes, and permit the design of products and processes that better utilize scarce resources, are significantly less polluting, and are much safer.
- Future computer aids will allow design and control engineers to examine many more alternatives much more thoroughly and thus produce better solutions to problems within the known technology.
- Better modeling will allow the design of processes that are easier and safer to operate. Improved control methodology and sensors will overcome the current inability to model certain processes accurately.

- Sensors of the future will be incredibly small and capable. Many will feature a chemical laboratory and a computer on a chip. They will enable chemical engineers to detect chemical compositions inside hostile process environments and revolutionize their ability to control processes.

To realize the promise of the computer in chemical engineering, we need a much larger effort to develop methodologies for process design and control. In addition, state-of-the-art computational facilities and equipment must become more widely disseminated into chemical engineering departments in order to integrate methodological advances into the mainstream of research and education.

Surface and Interfacial Engineering (Chapter 9)

Surfaces, interfaces, and microstructures play an important role in many of the above-mentioned research frontiers. Chemical engineers explore structure-property relationships at the atomic and molecular level, investigate elementary chemical and physical transformations occurring at phase boundaries, apply modern theoretical methods for predicting chemical dynamics at surfaces, and integrate this knowledge into models that can be used in process design and evaluation. Fundamental advances in these areas will have a broad impact on many technologies. Examples include laying down thin films for microelectronic circuits, developing high-strength concrete for roadways and buildings, and inventing new membranes for artificial organs. Advances in surface and interfacial engineering can also move the field of heterogeneous catalysis forward significantly. New knowledge can help chemical engineers play a much bigger role in the synthesis and modification of novel catalysts with enhanced capabilities. This activity would complement their traditional strength in analytical reaction engineering of catalysts.

Process Design in the Twenty-First Century

It is April 2008. A process engineer for a medium-size chemical company is designing a process for a new series of products her company is developing. By talking to her computer and pointing to the screen, she indicates that the process needs a reactor and some separators, as well as recycle capability. The computer, using an artificial intelligence program tied to mathematical models, determines how the reactor temperature and pressure will influence the product yield and how the yield in turn will influence the amount of process stream recycle. The computer chooses the optimal types of separators and indicates how they should be sequenced for different process streams and products of the series. After 20 minutes on a teraflop machine (equivalent to 1 week of computing on a CRAY I of the 1980s), the computer indicates that the general process design is completed, having given partial information during the 20-minute computation.

The graphical output from the computer shows the process flowsheet, with several separation units and projected equipment and operating costs. It also flags information that is uncertain because it had to use thermodynamic data extrapolated from measured values. At the engineer's request, the computer shows several alternative flowsheets it had considered, indicates their projected costs, and tells why it eliminated each of them. Some of the flowsheets were eliminated because of high cost, others because they were considered unsafe, others because the startup procedures would be difficult, and still others because they were based on uncertain extrapolation of experimental data.

After perusal of these process options, the engineer asks the computer to select five designs for further study, and the computer produces a paper copy of the flowsheet and design parameters

HIGHLIGHTS OF THE RECOMMENDATIONS

Education and Training of Chemical Engineers (Chapter 10)

The new research frontiers in chemical engineering, some of which represent new applications for the discipline, have important implications for education. A continued emphasis for each. She then tells the computer to prepare more rigorous designs from each of the five flowsheets. The computer questions the third design because it involves extensive extrapolation of known thermodynamic data. However, since the engineer feels that this may be one of the better designs, she asks the computer to notify the laboratory management computer, which is connected to a laboratory technician's terminal, to begin experimental determination of the missing thermodynamic data so that the design can be prepared. The computer reminds the engineer that precise control of process conditions will be essential for another of the designs and that a new sensor may be necessary. The engineer consults the artificial intelligence program, which tells her that either of two sensors being developed in another company department may fill her need. The engineer uses her computer to schedule a meeting with the manager of sensor development.

Two years later, the process is producing one of the new products. The time from design to operation was short enough that the company was able to capture a new market, and it plans to extend the business with related products of the series. The design engineer has developed a real-time dynamic simulation of the process and is running it on the company's parallel supercomputer to test strategies for producing the related products from the process equipment. She queries another computer to search the historical records of plant operation. She wants to know how well the dynamic model mirrors the actual plant operation and when the process parameters were last adjusted to give a better fit between the plant and the model. She feeds this information to her computer, which devises a comprehensive plan for scheduling production among four of the new products, with provision for revising the schedule as inventories and sale of the products change. A new business has been created.

is needed on basic principles that cut across many applications, but a new way of teaching those principles is also needed. Students must be exposed to both traditional and novel applications of chemical engineering. The American Institute of Chemical Engineers (AIChE) has set in motion a project to incorporate into undergraduate chemical engineering courses examples and problems from emerging applications of the discipline. The committee applauds this work, as well as recent AIChE moves to allow more flexibility for students in accredited departments to take science electives.

A second important need in the curriculum is for a far greater emphasis on design and control for process safety, waste minimization, and minimal adverse environmental impact. These themes need to be woven into the curriculum wherever possible. The AIChE Center for Chemical Process Safety is attempting to provide curricular material in this area, but a larger effort than this project is needed. Several large chemical companies have significant expertise in this area. Closer interaction between academic researchers and educators and industry is required to disseminate this expertise.

The Future Size and Composition of Academic Departments (Chapter 10)

A bold step by universities is needed if their chemical engineering departments are (1) to help the United States achieve the preeminent position of leadership in new technologies and (2) to keep the highly successful U.S. chemical processing industries at the forefront of world markets for established technologies. The universities should conduct a one-time expansion of their chemical engineering departments over the next 5 years, exercising a preference for new faculty capable of research at interdisciplinary frontiers.

This expansion will require a major commitment of resources on the part of universities, government, and industry. How can such a preferential commitment to one discipline be justified, particularly at a time of budgetary austerity? One answer is that the worldwide contest for dominance in biotechnology, advanced materials technologies, and advanced information devices is in full swing, and the United States cannot afford to stand by until it gets its budgetary house in order. As the uniquely "molecular" engineers, chemical engineers have powerful tools that need to be refined and applied to the commercialization of these technologies. A second answer is that the alternative to expansion, a redistribution of existing resources for chemical engineering research, would cut into vital programs that support U.S. competitiveness in established chemical technologies. The recommendation for an expansion in chemical engineering departments is not a call for "more of the same." It is the most practical way to move chemical engineering aggressively into the new areas represented by this report's research priorities while maintaining the discipline's current strength and excellence.

Balanced Portfolios (Chapter 10)

The net result of an additional investment of resources in chemical engineering should be the creation of three balanced portfolios: one of priority research areas, one of sources of funding for research, one of mechanisms by which that funding can be provided.

The eight priority research areas described above constitute the committee's recommendation of a balanced portfolio of research areas on the frontiers of the discipline.

In terms of a balanced portfolio of funding sources, the committee proposes initiatives for industry and a number of federal agencies in Chapter 10 and Appendix A to ensure a healthy diversity of sponsors. Table 1.1 links specific research frontiers to funding initiatives for potential sponsors.

A third balanced portfolio, of funding mechanisms, is needed if the above-mentioned research frontiers are to be pursued in the most effective manner. Different frontiers will require different mixes of mechanisms, and the decision to use a particular mechanism should be determined by the nature of the research problem, by instrumentation and facilities requirements, and by the perceived need for trained personnel in particular areas for industry. This topic is discussed in more detail in Chapter 10.

The Need for Expanded Support of Research in Chemistry (Chapter 10)

Chemical engineering builds on research results from other disciplines, as well as those from its own practitioners. Not surprisingly, the most important of these other disciplines is chemistry. A vital base of chemical science is needed to stimulate future progress in chemical engineering, just as a vital base in chemical engineering is needed to capitalize on advances in chemistry. The committee endorses the recommendations contained in the NRC's 1985 report *Opportunities in Chemistry*, and urges their implementation in addition to the recommendations contained in this volume.

TABLE 1.1 High-Priority Research Frontiers and Initiatives[a]

	Relevant Audiences[b]							
Priority Research Area	NSF	DOE	NIH	DOD	EPA	NBS	BOM	CPI
Biotechnology and biomedicine	✓✓	✓	✓✓			✓		✓
Electronic, photonic, and recording materials and devices	✓✓	✓		✓✓				✓
Microstructured materials	✓✓	✓✓		✓✓		✓		✓
In-situ processing of resources	✓	✓✓					✓✓	
Liquid fuels for the future	✓	✓✓				✓		✓✓
Responsible management of hazardous substances	✓				✓✓			✓✓
Advanced computational methods and process control	✓✓	✓						✓✓
Surface and interfacial engineering	✓✓	✓✓						✓

[a] ✓✓ = major initiative recommended; ✓ = supporting initiative recommended.

[b] NSF = National Science Foundation; DOE = Department of Energy; NIH = National Institutes of Health; DOD = Department of Defense; EPA = Environmental Protection Agency; NBS = National Bureau of Standards; BOM = Bureau of Mines; CPI = U.S. chemical processing industries.

Recommendations

THE FULL REPORT from which this summary volume has been excerpted discusses future contributions of chemical engineering research to the national well-being, and emerging research frontiers of special importance. Implications of these frontiers are explored in individual chapters of that report. This section, which appears as Chapter 10 in the full report, details the committee's principal recommendations for action by policymakers in four sectors: academia, industry, government, and professional societies.

RECOMMENDATIONS FOR ACADEMIA

Education and Training of Chemical Engineers

Chemical engineering undergraduate curricula have traditionally been designed to train students for employment in the conventional chemical processing industries. The current core curriculum is remarkably successful in this effort. Chemical engineers will continue to play a major role in the chemical and petroleum industries, but new areas of application as well as new emphases on environmental protection; process safety; and advanced computation, design, and process control will require some modifications of the curriculum.

These modifications will not entail a massive revision of the curriculum. Continued emphasis is needed on basic principles (e.g., thermodynamics, transport, separations, reaction engineering, and design) that cut across many applications. A new way of teaching these principles, though, is needed. Students must be exposed to both traditional and novel applications of chemical engineering. Emerging applications should be highlighted and an expanded range of process scales and chemistries should be taught.

Chemical engineers of the future will not only be interested in the transport and chemical reactions of small molecules, they will also be interested in large organic molecules, proteins, polymers (both organic and inorganic), interfaces, surfaces, solids, and composite materials. Each of these new areas of interest—to the extent that they are not now treated in depth in the core courses of the curriculum—will require new emphasis in teaching. Core courses such as separations (unit operations) should be modified to cover new challenges. For example, a separations course might be modified to address challenges in bioprocessing and ultrapurification by placing less emphasis on distillation, absorption, and conventional extraction, and more emphasis on examples of generating high selectivity in separations.[1] Similarly, unit operations laboratories and design courses in most institutions are in need of rethinking and revitalization to accommodate new needs and opportunities.

Another important need in the curriculum is for a far greater emphasis on design and control for process safety, waste minimization, and minimal adverse environmental impact. These themes need to be woven into the curriculum wherever possible. The AIChE Center for Chemical Process Safety is attempting to provide curricular material in this area, but a larger effort than this project is needed. Several large chemical companies have significant expertise in this area. They should do more to share their insights and methods with academic researchers and educators. (See "Recommendations for Industry" below.)

Certain emerging specialties of chemical engineering will require a deeper exposure of engineering students to other disciplines. For example:

- Students interested in biochemical engineering will need to become familiar with the language and concepts of the biosciences, particularly molecular biology.
- For those interested in advanced materials, more in-depth knowledge of statistical and continuum mechanics than that provided in transport and mechanics courses seems essential.
- Chemical engineering students interested in electronic materials need to understand the elements of electrical engineering and solid-state physics in order to work productively with colleagues in these disciplines.
- A greater emphasis on surface and interfacial phenomena is needed for all chemical engineers interested in materials engineering.

It will not be easy to shoehorn all of these elements into a 4-year program, and the initiation of carefully delimited electives and options for students is probably the best approach. The committee applauds the recent action of the AIChE to allow more flexibility in the choice of science electives by undergraduates in accredited departments. The development of such electives and new course sequences is not a casual undertaking. It will require greater involvement by faculty in providing advice and counsel to students at the sophomore and junior level, who may not be prepared to make definitive choices about specialization in their major.

The mix of industries into which chemical engineers are being introduced is changing rapidly. Chemical engineers have always been proud of their flexibility and willingness to rise to new challenges. This characteristic will be needed more than ever in the future, as diversity within the processing industries greatly increases. For example:

- There will be a greater variety of products and processes, increased demands for quality, and a necessity to shift rapidly from one product to another.
- Chemical engineers will be working with a broader spectrum of colleagues; not just chemists, but physicists, biologists, material scientists, and electrical engineers.
- The chemical processing industries will increasingly have a global outlook. They will require engineers with a greater understanding of the global economy, knowledge of engineering research and advances occurring in other countries, and appreciation for other cultures.
- These industries will be operating in a complex regulatory environment. They will require engineers with a broad perspective on economic and environmental policy.
- With the current emphasis on streamlined management and lean staffing in the processing industries, chemical engineers will have fewer peers and fewer superiors; their ability to make decisions will be tested early and often.

Chemical engineering faculty need to consider these challenges in planning for tomorrow's more broadly based curriculum.

The Future Size and Composition of Academic Departments

How can chemical engineering departments implement this broader curriculum as well as aggressively respond to new research challenges and opportunities? A bold step is needed. The committee recommends that universities conduct a one-time expansion of chemical engineering departments over the next 5 years, exercising a preference for new faculty capable of research at interdisciplinary frontiers.

This expansion will require a major commitment of resources on the part of universities, government, private foundations, and industry. Can such a preferential commitment to one discipline be justified, particularly at a time of severe budgetary austerity? Each agency responsible for funding chemical engineering research will have to answer this question for itself, but the committee believes that the following arguments for expansion are worthy of consideration.

- U.S. leadership in technology is under challenge today as never before. Future markets for biotechnology products, advanced materials, and advanced information devices will be won by those who are best able to integrate innovative product design with efficient process design. Chemical engineers, the uniquely "molecular" engineers, have powerful tools that can be refined and applied to these challenges. They can make significant contributions to U.S. capabilities in these high-technology areas.
- The existing chemical processing industries are high-technology enterprises too. Moreover, the chemical industry is one of the most successful U.S. businesses on world markets, with a $7.8 billion trade surplus in 1986. The chemical processing industries require a continuing supply of highly trained engineers armed with the latest insights from the core research areas of chemical engineering. Redirecting resources from these established and vital research programs to the emerging applications of the discipline risks reducing the U.S. leadership position in a large and very successful industrial sector. Is this an appropriate course to take at a time when industrial competitiveness and the large U.S. trade deficit are such pressing national concerns?

Increasing the number of research groups and the resource levels in chemical engineering seems to be the most practical way of (1) moving chemical engineers aggressively into the new areas among this report's research priorities while (2) maintaining the discipline's current strength and excellence. The following recommendations for universities are intended to ensure that such an additional investment would be used most effectively.

- Chemical engineering departments should use additional resources to appoint and encourage faculty to address the frontier areas identified in this report. Many of these frontiers will require more emphasis on interdisciplinary research. The selected appointment of chemical engineering faculty whose backgrounds are from other disciplines should be encouraged. Cross-disciplinary research partnerships between chemical engineers and colleagues in other disciplines should be stimulated and rewarded. (See sections on "Cross-disciplinary pioneer awards" and "Cross-disciplinary partnership awards" under "Recommendations for Government.")
- Academic institutions should make substantial commitments (e.g., cost sharing and maintenance for instrumentation and facilities) to make additional outside investments in chemical engineering as effective as possible. This will be particularly needed to justify additional funding for advanced instrumentation and computers needed for research and education.
- Academic institutions should take steps to strengthen even further the long-standing ties between chemical engineering departments and the chemical process industries, with a particular emphasis on the flow of personnel between academia and industry. Mechanisms for increased interchange include appointing industrial researchers as adjunct professors in academia; providing industrial sabbaticals for university professors; inviting industrial researchers as seminar speakers on campus; placing student researchers in industrial laboratories; and expanding the number of masters degree programs, evening continuing education programs, and other short courses targeted to industrial research personnel.
- Chemical engineering departments should use additional resources to promote greater interaction with small process technology firms in electronics and biotechnology. These firms play an unheralded but key role in developing industrial technology in these areas. (See section on "Improvment of Links Between Chemical Engineering Departments and Small Process Technology Firms" under "Recommendations for Government.")

RECOMMENDATIONS FOR INDUSTRY

The committee encourages decision makers in the chemical processing industries to increase their commitment to research in their companies both to retain their world leadership in chemical technology and to deliver to American society the maximum benefit from its investment in basic research. These industries should take advantage of opportunities to leverage their funds by working cooperatively in generic nonproprietary areas, such as in-situ processing and solids processing (see Chapter 6) and health, safety, and environmental protection (see Chapter 7).

Industry should also continue to commit resources to academic research, for reasons that go far deeper than the desirability of additional funds. The development of any engineering field, and particularly one as closely linked to manufacturing as chemical engineering, needs the intellectual guidance that can only come from an industry with a stake in research outcomes. Also, industry has to be linked to academia so that new laboratory results can be rapidly transferred to product and process design. An industry committed to financial sponsorship and personnel exchanges with academia will make sure that the crucial industrial intellectual involvement needed for success exists. Thus, the committee urges that:

- industry continue to expand its support of academic chemical engineering research in each of the priority research areas identified in this report, and
- policymakers in industry facilitate the exchange of people across the interface between industry and academia. A number of possible mechanisms for such exchanges are listed under "Recommendations for Academia." They will be most effective if corporate culture and incentive structures promote their use.

One area where intellectual involvement from industry is essential is in the area of process safety. Several large chemical companies have committed substantial resources to advancing their understanding of this area. The generic insights that have emerged from their work need to be shared with academic researchers and educators as well as with smaller chemical companies. The AIChE Center for Chemical Process Safety is one mechanism for such transfer of knowledge. Others, including direct contacts between academic and industrial researchers in this area, are needed.

RECOMMENDATIONS FOR GOVERNMENT

Balanced Portfolios

Maintaining the health of any research enterprise is a difficult undertaking. It requires the recognition that future research advances are unpredictable and may substantially reorder the initial priorities assigned to particular areas of investigation. It demands an appreciation that researchers in different areas of science and technology, or at different stages in their professional careers, have different requirements for support. To maintain the vitality of chemical engineering, and to enable its researchers to make the greatest possible contribution to solving the problems facing society, a philosophy of support for the field is needed that can be described in terms of three "balanced portfolios": one of priority research areas, one of funding sources, and one of support mechanisms.

The committee's balanced portfolio of priority research areas has already been discussed in the Executive Summary and the preceding chapters of the report. A discussion of the portfolio of funding sources is presented later in this chapter and, more comprehensively, in Appendix A. The following section discusses the committee's views on a balanced portfolio of support mechanisms.

Support Mechanisms in Perspective

Different research frontiers require different mixes of support mechanisms. The appropriate mix for a particular area depends on several of factors, including the nature of the scientific area; its requirements for expensive equipment, instruments, or facilities; and the need for trained personnel from that area in the broader economy.

Table 10.1 summarizes a range of possible funding mechanisms for research. Many of these mechanisms are well known and need no further elaboration. Some of them, however, are new

TABLE 10.1 Possible Support Mechanisms for Chemical Engineering Research

Support Mechanism	Likely Applicants
Single Investigator Awards	
Starter grants	New investigators
Presidential Young Investigator awards	Extremely promising new investigators
Solicited project grants	Younger investigators
Career development awards	Extremely meritorious younger investigators
Unsolicited project grants	Meritorious midstream investigators
Research excellence awards[a]	Extremely meritorious midstream and senior investigators
Cross-Disciplinary Awards	
Cross-disciplinary pioneer awards[a]	Promising new investigators moving into chemical engineering from other disciplines
Cross-disciplinary partnership awards[a]	Two or three co-principal investigators from different disciplines
Equipment and Facilities	
Equipment and instrumentation grants	One or more research groups with special requirements
Regional and national equipment facilities	Researchers needing unique equipment or facilities (e.g., synchrotrons or neutron-scattering facilities)
Centers and Academic-Industrial Consortia	
NSF Science and Technology Centers	Collaborative groups of principal investigators
Engineering Research Centers	Cross-disciplinary engineering research attacking several aspects of a common problem area
Other centers and consortia[a]	Large research centers funded by industry at academic campuses
Improving links to small high-technology firms[a]	Small process technology companies working with academic investigators

[a] New mechanism discussed in the text.

mechanisms being proposed by the committee and are briefly described below. The great number of possible support mechanisms for academic research should be placed in the following perspective. Academic chemical engineering research has traditionally taken place in the milieu of the small research group. Led by a single principal investigator, the small research group enjoys the advantages of decentralization and freedom to move in new directions as opportunities unfold. The environment of a small group, where individual students take responsibility for significant parts of the group's work, also facilitates the training of independent-minded researchers. Much of the vitality of chemical engineering over its history can be traced to innovative and exploratory research groups led by individual principal investigators.

Because of these advantages, and because many of the most exciting research problems in chemical engineering lend themselves to the scale and environment of the small research group, grants to individual academic investigators should continue to remain the mainstay of the portfolio of support mechanisms for research.

In this context, how can interdisciplinary research on the frontiers of the discipline be best conducted? There is an obvious role for large assemblages of researchers in centers of various types. The committee also believes that the special needs of interdisciplinary research areas can be met by creating two new funding mechanisms modeled on the best features of the small group model.

- Cross-disciplinary partnership awards. The first mechanism is designed to encourage two or three small research groups to submit joint applications. Such a partnership would allow researchers to begin to communicate across disciplinary lines without necessitating the creation of large and formal entities. Cross-disciplinary partnership awards should be considered in a variety of research areas where interdisciplinary cooperation is vital, and particularly for research areas associated with the emerging technologies described in Chapters 3 through 5 of this report. A case in point would be biochemical engineering where more effective links in research and training are needed between chemical engineers and life scientists.
- Cross-disciplinary pioneer awards. The second proposed interdisciplinary funding mechanism would encourage top-quality young researchers trained in other disciplines (e.g., molecular biology, chemistry, materials science, and solid-state physics) to accept tenure-track faculty positions in chemical engineering departments at leading universities. The committee believes that these departments would be enriched by the judicious appointment of young faculty who are at the cutting edge of disciplines important to chemical engineering frontiers. Some appointments of this type are already being made, but these "pioneers" face considerable obstacles in making the transition from the area in which they were trained to another discipline. These include the difficulties of immersing oneself in a new intellectual area while trying to start a new research program that, absent a track record of success, might not appeal to proposal reviewers (whether from the pioneer's initial or newfound field) who have the more traditional perspectives of established disciplines. The proposed award would provide start-up research funding for up to 5 years. It would allow cross-disciplinary pioneers the opportunity to engage themselves fully in chemical engineering research so that they can, by the end of the award, contribute to the intellectual life and teaching of their department and compete with other researchers on an equitable basis in the peer-review system.

Another type of individual award that belongs in the balanced portfolio is the type of "research excellence award" exemplified in the industrial world by the IBM Fellows program. These awards would choose extremely meritorious investigators for research support on the basis of the creativity, productivity, and significance of their recent work, rather than on the basis of a project proposal. They would be provided funding for themselves and one or two associates to pursue topics entirely of their own choosing for a few years. The result is that a select group of the very best researchers would receive the opportunity to explore more speculative and high-potential research ideas. An experimental program of this type, with no more than 15 such awards active at any given time, might leaven the entire field of chemical engineering as well as provide an award that would represent the pinnacle of individual personal achievement in chemical engineering research.

Like other fields where the small research team is still a vital and appropriate funding mode (e.g., chemistry, solid-state physics, biology, and materials science), the cost of conducting cutting-edge research in chemical engineering is growing rapidly. The complex problems of the future will require groups that are larger than today's. The cost of state-of-the-art instrumentation and computational facilities will continue to grow. It is important that agencies funding chemical engineering research ensure that their investment is made most effective by realistically estimating the research costs of groups at the frontiers of the field. In Appendix A (Table A.2), the committee provides its own estimates of appropriate levels of support for groups of different sizes. *Particularly in a time of budgetary stringency, research effectiveness is maximized by funding a smaller number of excellent projects adequately, rather than funding a large number of projects inadequately.*

Funding agencies with vital programs of support for small research groups may also want to consider implementing one of the following two mechanisms:

- Improvement of links between chemical engineering departments and small process technology firms. There is an important problem in some key emerging technologies that is not addressed by existing programs in funding agencies. It deals with the generation and transfer of expertise and ideas from the research laboratory to the production line in biotechnology and process technologies for electronic, photonic, and recording materials and devices. In these areas, a key role in generating new process concepts and equipment is played by a large number of relatively small firms. These firms are capital-poor but rich in problems that would benefit markedly from the insights of academic chemical engineers. The United States could significantly boost its competitive position in these areas by facilitating information transfer between academia and this segment of industry. The problem for funding agencies with an interest in promoting U.S. capabilities in this area is how to create incentives for academic and industrial researchers to seek out and forge links with one another.

Since the partnerships that the committee would like to see fostered will be individualized to the particular needs and interests of the participants, it recommends that any program allow for a wide range of proposed activities. Examples of the sort of initiatives that might be funded include the following: (1) Grants to provide special instrumentation and facilities to be shared by researchers from chemical engineering departments and high-technology firms. The facilities or instrumentation would be located at a university, but available for use by researchers at the high-technology firms. (2) Sabbatical awards for academics at smaller firms that are not likely to be plugged into a large university-based center. (3) Starter grants for industrial liaison programs in chemical engineering departments. A key feature of any of these programs would have to be significant, ongoing person-to-person contact between academic and industrial research groups.

- Large research centers and consortia. Centers are a common mechanism of support to stimulate cross-disciplinary interactions among researchers, or to facilitate cooperation among researchers from academic, industrial, and government laboratories. The most ambitious program of support for cross-disciplinary centers is the NSF Engineering Research Centers (ERC) Program. The cross-disciplinary character of many of the frontiers discussed in this report makes them fruitful areas for center-based research, a reality that has already been recognized by the establishment of ERCs in bioprocess engineering, compound semiconductors, composite materials, hazardous waste management, and computer-assisted design. Chemical engineering plays an important role in nearly all these centers.

Federal agencies should try to stimulate industrial interest in consortia to bolster U.S. industrial competitiveness in technology. DOE should foster interest in consortia to address macroscale energy and natural resource processing research. Here, the experiments that need to be carried out are large in size and costly. Generic research could be carried out in such a way as to (1) allow for fruitful interchange of ideas between industrial and academic researchers, and (2) improve the research efficiency of the energy processing industries while leaving each individual energy company free to develop its own proprietary processes and technologies.

Another area where the large-scale approach might be appropriate is in research related to environmental protection and process safety. Again, this is an area where cooperation on generic research among companies, as well as between industry and academia, focused on generic research problems, could lead to significant research advances. EPA should stimulate such arrangements to expand the amount of research needed in this area.

Recommendations for Specific Federal Agencies

National Science Foundation

The NSF has a logical role in each of the following frontier research areas.

- Biotechnology. NSF should sustain the growth and quality of its existing research support. This area should be targeted by NSF for new support to cross-disciplinary pioneers, partnerships, instrumentation and facilities, and incentives to improve links between academia and small high-technology firms.

- Electronic, photonic, and recording materials and devices. The committee recommends a 5-year pattern of budgetary growth to achieve 30 groups funded at an aggregate level of $6 million per year. This area should also be targeted for new support to cross-disciplinary pioneers and improved links between academia and small high-technology firms.
- Polymers, ceramics, and composite materials. In addition to continued growth in support for research on polymers and polymeric composites, a new thrust is recommended to establish six to eight centers for the chemical engineering of ceramic materials and composites over the next 5 years, funded at a total annual level of $4 million per year. Cross-disciplinary pioneers should also be supported in this area.
- Energy and natural resources processing. NSF should sustain its support of basic research in complex behavior in multiphase systems, catalysis, separations, dynamics of solids transport and handling, and new scale-up and design methodologies.
- Environmental protection, process safety, and hazardous waste management. NSF should strongly support continued growth in this area, focusing on engineering design methodology for process safety and waste minimization. In FY 1986, only three chemical engineering groups working in this area were funded by NSF, with total support of less than $250,000.
- Computer-assisted process and control engineering. The committee recommends a 5-year pattern of growth to a program supporting 35 groups with access to state-of-the-art workstations, software, and computer networks, in addition to the existing ERCs related to this topic. These smaller groups should receive total NSF support of $8 million per year as a target, keyed to additional industry support.
- Surface and interfacial engineering. NSF should expand its support in this area, with emphasis on acquisition by chemical engineers of state-of-the-art instrumentation for surface and interface studies. The need for such dedicated instrumentation can be met at a funding level of about $5 million per year.

To make its most effective contribution to these areas, NSF should consider the following new support mechanisms for chemical engineers.

- Cross-disciplinary pioneer awards. The committee recommends that a steady-state program of 25 awards in the three areas identified above be achieved over 5 years, at an average level of $100,000 per award per year.
- Research excellence awards. The committee recommends that a steady-state program of 15 awards be achieved over 3 years, at an average level of $100,000 per year per award.
- Improvment of links between chemical engineering departments and high-technology firms. NSF should create a program to provide incentives for partnerships between academic chemical engineers and researchers in small process technology firms in biotechnology and electronics.

Department of Energy

The committee proposes major new initiatives for the Office of Fossil Energy and the Office of Energy Research (OER) to support in-situ processing of resources and the development of tomorrow's liquid fuels for transportation. New initiatives are also proposed for these two offices for the chemical engineering of advanced materials and the development of computational methods and process control—the Division of Engineering and Geosciences in OER should continue and expand its support of fundamental research while the Office of Fossil Energy starts a new program on applications to design and scale-up of large-scale technologies. An initiative is recommended for the Office of Energy Utilization in chemical and process engineering for biotechnology applied to renewable sources for chemical feedstocks. Surface and interfacial engineering should be a strong focus for initiatives in the OER (e.g., catalysis and colloid science and technology) and in Office of Energy Utilization (e.g., corrosion science and electrochemical engineering).

National Institutes of Health

The committee recommends that the National Institutes of Health undertake an initiative to support basic research in chemical and process engineering science for ultimate application to biotechnology and biomedical devices. Such an initiative should be targeted towards (1) encouraging the submission of proposals from cross-disciplinary partnerships between chemical engineers and life scientists and (2) shaping the biochemical engineers of the future by using Institutional National Research Service Awards to facilitate the expansion of graduate course requirements in biochemical engineering to include greater exposure to the life sciences.

Department of Defense

For the Department of Defense (DOD), the main relevant thrust of this report is in the area of materials. The committee recommends that DOD formulate integrated initiatives (i.e., from basic research to testing and evaluation) on problems where improvements in chemical processing is the key to enhanced performance (e.g., polymer-based optical fiber and components; processing for high-strength, high-modulus fibers; manufacturing process technology for composite materials; and joining and repair science for complex materials systems based on polymers, ceramics, and composites). At the basic end of the research spectrum, these would translate into major initiatives for support of molecular science and engineering.

Environmental Protection Agency

The Environmental Protection Agency should revitalize its research grant program in its Office of Exploratory Research and substantially increase its support to chemical engineers investigating important challenges to environmental quality. Stability in the EPA research program over several years is needed to attract the best scientific and engineering research talents to these problems and to allow them to work efficiently on their solution.

The EPA should also consider creating a national "Center for Engineering Research on Environmental Protection and Process Safety" that would provide both unique state-of-the-art laboratory facilities and computational resources to chemical and process engineering researchers from academia, federal laboratories, and industry.

National Bureau of Standards

The small NBS program in chemical engineering should receive substantially greater funding to fulfill critical needs for evaluated data and predictive models. The committee supports NBS plans to focus on data needs in emerging technology areas such as biotechnology and advanced materials.

Bureau of Mines

The committee recommends that the Bureau fund a modest number of university-based centers focused on in-situ processing of dilute resources. This initiative would complement the one proposed for DOE.

RECOMMENDATIONS FOR PROFESSIONAL SOCIETIES

American Institute of Chemical Engineers

Every constituent part of the American Institute of Chemical Engineers (AIChE)—journals, committees, local sections, divisions, and student chapters—should take up the challenge of examining the frontiers presented in this report and, in the context of their mission and purpose, seek out ways of rejuvenating the profession of chemical engineering. The AIChE should set the goal of attracting to its meetings and journals significant numbers of researchers from other disciplines who are working on problems closely related to those at the cutting edge of chemical engineering. The AIChE should provide awards that recognize the achievements of chemical engineering researchers in frontier areas. The Institute should continue to promote reforms in chemical engineering education that would ensure that students would be prepared for new challenges. Finally, the AIChE should undertake cooperative efforts with other organizations to advance the interests of the discipline.

American Chemical Society

Over 12,000 chemical engineers are members of the American Chemical Society (ACS), and ACS meetings, journals, and abstracting services support chemical engineering research in important ways. The findings and recommendations of this report should be of interest and concern to the ACS. In fact, many of the recommendations for the AIChE could be profitably implemented by the ACS Divisions of Industrial and Engineering Chemistry, Colloid and Surface Chemistry, Environmental Chemistry, Fuel Chemistry, Microbial and Biochemical Technology, Polymer Chemistry, and Polymeric Materials: Science and Technology. The ACS is a natural partner for the AIChE in joint undertakings to benefit chemical engineering, and the committee recommends that both organizations pursue opportunities for future joint undertakings.

Council for Chemical Research

The Council for Chemical Research is not a professional society, but it deserves mention in this report because it provides a unique forum for interactions between chemists and chemical engineers and between academic and industrial researchers. It should be credited for building effective bridges among these groups and highlighting the complementary character of chemistry and chemical engineering. The committee recommends that the Council continue to promote these interactions, focusing on the frontiers in this report that are of mutual interest to chemists and chemical engineers.

CONCLUSION

Chemical engineering is a discipline that integrates the research advances of a number of scientific areas. Paramount among these is chemistry, but fields such as applied mathematics, biology, computer science, condensed-matter physics, environmental science and engineering, and materials science also provide important insights that chemical engineers use. This report has highlighted chemical engineering accomplishments and frontiers in a number of areas touched on by these disciplines. It is important that these other disciplines remain vital, too.

Two years ago, the National Research Council published a report on research frontiers and needs in chemistry entitled *Opportunities in Chemistry*, and known colloquially as the "Pimentel Report" after its chairman, George C. Pimentel. That report identified many of the same areas discussed in this report and focused on how chemists contribute to them. This committee endorses the recommendations contained in *Opportunities in Chemistry*, and urges their implementation in addition to the recommendations contained in this volume. The two reports, like the two disciplines, should be seen as complementary, not competing. A vital base of chemical science is needed to stimulate future progress in chemical engineering, just as a vital base in chemical engineering is needed to capitalize on advances in chemistry.

NOTE

1. More detailed suggestions for education and training in separations are contained in *Separations and Purification: Critical Needs and Opportunities* Washington, D.C.: National Academy Press, 1987.

EXECUTIVE SUMMARY

Reprinted from

OPPORTUNITIES IN Chemistry

Committee to Survey Opportunities in the Chemical Sciences
Board on Chemical Sciences and Technology
Commission on Physical Sciences, Mathematics, and Resources
National Research Council

NATIONAL ACADEMY PRESS
Washington, D.C. 1985

NATIONAL ACADEMY PRESS 2101 Constitution Avenue, N.W. Washington, D.C. 20418

NOTICE: The project that is the subject of this report was approved by the Governing Board of the National Research Council, whose members are drawn from the councils of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine. The members of the committee responsible for the report were chosen for their special competences and with regard for appropriate balance.

This report has been reviewed by a group other than the authors according to procedures approved by a Report Review Committee consisting of members of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine.

The National Research Council was established by the National Academy of Sciences in 1916 to associate the broad community of science and technology with the Academy's purposes of furthering knowledge and of advising the federal government. The Council operates in accordance with general policies determined by the Academy under the authority of its congressional charter of 1863, which establishes the Academy as a private, nonprofit, self-governing membership corporation. The Council has become the principal operating agency of both the National Academy of Sciences and the National Academy of Engineering in the conduct of their services to the government, the public, and the scientific and engineering communities. It is administered jointly by both Academies and the Institute of Medicine. The National Academy of Engineering and the Institute of Medicine were established in 1964 and 1970, respectively, under the charter of the National Academy of Sciences.

Support for this project was provided by the American Chemical Society, the U.S. Air Force Office of Scientific Research under Grant No. AFOSR-83-0323, the Council for Chemical Research, Inc., the Camille and Henry Dreyfus Foundation, Inc., the U.S. Department of Energy under Grant No. DE-FG02-81ER10984, the National Institutes of Health under Grant No. CHE-8301035, the National Bureau of Standards under Contract No. NB835BCA2075, and the National Science Foundation under Grant No. CHE-8301035. Support was also provided by the following industrial companies: Aluminum Company of America, AT&T Bell Laboratories, Calgon Corporation, Celanese Research Company, Dow Chemical Company, Eastman Kodak Company, E.I. du Pont de Nemours and Company, Inc., Exxon Corporation, General Electric Company, GTE Laboratories, Inc., Johnson and Johnson Company, Mobay Chemical Company, Mobil Research and Development Corporation, Monsanto Company, Pfizer, Inc., Phillips Petroleum Company, PPG Industries, Inc., Proctor and Gamble Company, Shell Development Company, Standard Oil Company (Ohio), Stauffer Chemical Company, TRW, Inc., and U.S. Steel Corporation.

The complete volume of *Opportunities in Chemistry* from which this Executive Summary and these vignettes are extracted, is available for sale from the National Academy Press, 2101 Constitution Avenue, NW, Washington, DC 20418.

Printed in the United States of America

First Printing, October 1985
Second Printing, February 1986

Committee to Survey Opportunities in the Chemical Sciences

GEORGE C. PIMENTEL *(Chairman)*, University of California, Berkeley
ALLEN J. BARD, The University of Texas at Austin
FRED BASOLO, Northwestern University
JOHN H. BIRELY, Los Alamos National Laboratory
JOHN I. BRAUMAN, Stanford University
HARRY G. DRICKAMER, University of Illinois, Urbana
HANS-G. ELIAS, Dow Chemical Co.
MOSTAFA A. EL-SAYED, University of California, Los Angeles
DAVID A. EVANS, Harvard University
JOSEF FRIED, University of Chicago
GERHART FRIEDLANDER, Brookhaven National Laboratory
HARRY B. GRAY, California Institute of Technology
VLADIMIR HAENSEL, University of Massachusetts, Amherst
RALPH F. HIRSCHMANN, Merck Sharp & Dohme Research Laboratories
ISABELLA L. KARLE, U.S. Naval Research Laboratory
WILLIAM A. LESTER, JR., University of California, Berkeley
RUDOLPH A. MARCUS, California Institute of Technology
FRED W. McLAFFERTY, Cornell University
KOJI NAKANISHI, Columbia University
ALAN SCHRIESHEIM, Argonne National Laboratory
HOWARD E. SIMMONS, JR., E.I. du Pont de Nemours and Company, Inc.
WILLIAM P. SLICHTER, AT&T Bell Laboratories
GABOR A. SOMORJAI, University of California, Berkeley
EARL R. STADTMAN, National Institutes of Health
CHRISTOPHER T. WALSH, Massachusetts Institute of Technology
GEORGE M. WHITESIDES, Harvard University

WILLIAM SPINDEL, *Staff Director*, Board on Chemical Sciences and Technology (BCST)
ROBERT M. SIMON, *Staff Officer*, BCST
PEGGY J. POSEY, *Staff Officer*, BCST
ROBERT C. ROONEY, *Editor*, Commission on Physical Sciences, Mathematics, and Resources
MARTIN A. PAUL, *Technical Consultant*, BCST
MARY E. BUNDY, *Administrative Associate to Dr. Pimentel*
JEAN E. YATES, *Financial Coordinator*, BCST
RENEE R. HARRIS, *Senior Secretary*, BCST

Board on Chemical Sciences and Technology

ALLEN J. BARD *(Co-Chairman)*, The University of Texas at Austin
LEO J. THOMAS, JR. *(Co-Chairman)*, Eastman Kodak Company
FRED BASOLO, Northwestern University
STEPHEN J. BENKOVIC, Pennsylvania State University
JOHN H. BIRELY, Los Alamos National Laboratory
KENNETH B. BISCHOFF, University of Delaware
JOHN I. BRAUMAN, Stanford University
EUGENE H. CORDES, Merck Sharp and Dohme Research Laboratories
WILLIAM A. GODDARD III, California Institute of Technology
LOWELL P. HAGER, University of Illinois, Urbana
ARTHUR E. HUMPHREY, Lehigh University
DAVID W. McCALL, AT&T Bell Laboratories
FRED W. McLAFFERTY, Cornell University
LEO A. PAQUETTE, Ohio State University
GEORGE W. PARSHALL, E.I. du Pont de Nemours and Company, Inc.
GEORGE C. PIMENTEL, University of California, Berkeley
DAVID P. SHEETZ, Dow Chemical Co.
THRESSA C. STADTMAN, National Institutes of Health
MONTE C. THRODAHL, St. Louis, Mo.
NICHOLAS J. TURRO, Columbia University
GEORGE M. WHITESIDES, Harvard University

Commission on Physical Sciences, Mathematics, and Resources

HERBERT FRIEDMAN *(Chairman)*, National Research Council
CLARENCE R. ALLEN, California Institute of Technology
THOMAS D. BARROW, Standard Oil Company, Ohio (retired)
ELKAN R. BLOUT, Harvard Medical School
BERNARD F. BURKE, Massachusetts Institute of Technology
GEORGE F. CARRIER, Harvard University
CHARLES L. DRAKE, Dartmouth College
MILDRED S. DRESSELHAUS, Massachusetts Institute of Technology
JOSEPH L. FISHER, Office of the Governor, Commonwealth of Virginia
JAMES C. FLETCHER, University of Pittsburgh
WILLIAM A. FOWLER, California Institute of Technology
GERHART FRIEDLANDER, Brookhaven National Laboratory
EDWARD D. GOLDBERG, Scripps Institution of Oceanography
MARY L. GOOD, Signal Research Center, Inc.
J. ROSS MacDONALD, University of North Carolina
THOMAS MALONE, Saint Joseph College
CHARLES J. MANKIN, Oklahoma Geological Survey
PERRY L. McCARTY, Stanford University
WILLIAM D. PHILLIPS, Mallinckrodt, Inc.
ROBERT E. SIEVERS, University of Colorado
JOHN D. SPENGLER, Harvard School of Public Health
GEORGE WETHERILL, Carnegie Institution of Washington

RAPHAEL G. KASPER, *Executive Director*
LAWRENCE E. McCRAY, *Associate Executive Director*

Preface

In 1965, the National Research Council published *Chemistry: Opportunities and Needs*. This report, under the leadership of Frank Westheimer, surveyed the state of the discipline at that time. In the 20 years since then, chemistry has undergone a virtual revolution—in its techniques, instrumentation, and capabilities. New frontiers lie before us.

These new vistas made evident the need for a new survey of chemical science and its intellectual and economic impact. That need, recognized by the Chemistry Section of the National Academy of Sciences, led in 1982 to meetings of a planning committee formed by the Board on Chemical Sciences and Technology of the National Research Council. The planning committee unanimously recommended that a new survey of chemistry be undertaken, and the Board approved. This report is the result.

A committee of 26 members broadly representative of the academic, industrial, and governmental research sectors, the geographic areas, and the major subdisciplines of chemistry was appointed to guide the study and to frame its conclusions and recommendations. Put briefly, the committee's charge was to describe:

- the contemporary research frontiers of chemistry;
- the opportunities for the chemical sciences to address society's needs; and
- the resources needed to explore these frontiers to advance human knowledge and to exploit chemistry's opportunities to enhance the well-being of humankind.

To achieve these goals, the committee organized itself into five task forces, which, in turn, called upon more than 350 chemical researchers to suggest topics, prepare commissioned papers on cutting-edge research, and provide critiques of the manuscript as it developed. An interim report was prepared in the fall of 1983 under the auspices of the Academies' Committee on Science, Engineering, and Public Policy, titled *Report of the Research Briefing Panel on Selected Opportunities in Chemistry*. The briefing report was based upon research directions of particular promise that became apparent early in this study. Its recommendations and priorities are entirely consonant with the more complete analysis presented here.

Of course, a single report can hardly cover every aspect of a set of disciplines as broad, vigorous, and dynamic as the chemical sciences. For example, chemical engineering is not explicitly discussed in detail; a complementary examination of the research frontiers in that field is clearly called for and is now under way. Nevertheless, I believe that this report does indeed capture the essence of the chemical sciences today. Its content represents an enthusiastic consensus of a broad spectrum from that community. The National Research Council, the scientific community, and I, myself, are all deeply indebted to the members of the committee, the many colleagues who assisted the task forces, and the able and conscientious reviewers whose suggestions and advice determined the substance of the report. I am also grateful to my staff at Berkeley and to the staff of the NRC's Board on Chemical Sciences and Technology who, under the leadership of William Spindel, provided throughout the term of the project the day-to-day support so essential to its successful completion.

The primary audience for this report is made up of those responsible for guiding science policy in the Congress and the Administration. We believe that the report will be useful to other audiences, too: the leaders of the chemical industry, the chemical research community, and also those inquisitive about science, about its relation to their own lives, and about the current directions of that most central of sciences, chemistry.

GEORGE C. PIMENTEL
Chairman, Committee to Survey
Opportunities in the Chemical Sciences

The contents of the entire report, from which the executive summary and vignettes are extracted, are listed below.

Contents

I. INTRODUCTION 3

II. EXECUTIVE SUMMARY 6

III. CONTROL OF CHEMICAL REACTIONS 21
  A. New Processes, 21
  B. More Energy, 34
  C. New Products and Materials, 48
  D. Intellectual Frontiers, 63
  E. Instrumentation, 91

IV. DEALING WITH MOLECULAR COMPLEXITY 106
  A. More Food, 106
  B. Better Health, 125
  C. Biotechnologies, 144
  D. Intellectual Frontiers, 155
  E. Instrumentation, 178

V. CHEMISTRY AND NATIONAL WELL-BEING 193
  A. Better Environment, 193
  B. Continued Economic Competitiveness, 209
  C. Increased National Security, 222
  D. Intellectual Frontiers, 240
  E. Instrumentation, 265

VI. MANPOWER AND EDUCATION 279

VII. RESOURCES FOR BASIC RESEARCH IN THE CHEMICAL SCIENCES 288

APPENDIXES
  A. Chemistry in Industry: Tables, 327
  B. Contributors, 332

INDEX 337

CHAPTER II

Executive Summary

SOCIETAL BENEFITS FROM CHEMISTRY

Chemistry provides fundamental understandings needed to deal with many societal needs, including many that determine our quality of life and our economic strength.

New Processes

The U.S. chemical industry has a current $12 billion positive balance of trade. Continued competitiveness depends upon constant improvement of existing processes and introduction of new ones. Advances in chemical catalysis and synthesis will be key to maintaining our current position of world leadership. (See Section III-A.)

More Energy

Ninety-two percent of our present energy consumption is based upon chemical technologies; this will remain true well into the 21st century. However, new chemistry-based energy sources will have to be tapped. They will include low-grade fuels for which better control of chemical reactivity is needed so that we can protect the environment while providing energy at reasonable cost. (See Section III-B.)

New Materials

The next two decades will bring many changes in the materials we use, including the materials in which we are clothed, housed, and transported. Chemistry will play an increasingly vital role in this interdisciplinary field because advances will depend upon ability to tailor new substances, including polymers, to replace and outperform traditional or scarce materials. (See Section III-C.)

More Food

To increase world food supply, we need improvements in the production and preservation of food, soil conservation, and the use of photosynthesis. In collaboration with contiguous disciplines, chemistry plays a central role as we seek to clarify in detail the chemistry of biological life cycles. Once clarified they can be nurtured and controlled, while undesired side effects are avoided through chemical identification and synthesis of hormones, growth regulators, pheromones, self-defense structures, and nutrients. (See Section IV-A.)

Better Health

All life processes—birth, growth, reproduction, aging, mutation, death—are manifestations of chemical change. Chemistry is now poised to clarify such complex biological processes at the molecular level. Hence it is making important contributions to physiology and medicine through rational drug design and, then, through synthesis of new compounds that promote health and alleviate specific ailments such as atherosclerosis, hypertension, Parkinson's disease, cancer, and disorders of the central nervous and immune systems. (See Section IV-B.)

Biotechnologies

Remarkable progress made in recent years by molecular biologists and biochemists in genetic engineering has been built upon basic chemical principles that determine the chemical structures and functional relationships between molecules and supermolecules (proteins, DNA) within biological systems. Full realization of the potentialities of the projected new biotechnologies will increasingly depend upon molecular-level understandings. Chemists will be active collaborators in the progress toward this goal. (See Section IV-C.)

Better Environment

A major contemporary concern is protecting the environment in the face of increasing world population, urbanization, and rising standards of living. Effective strategies for safeguarding our surroundings require that we know what's there, where it came from, and what we can do about it. Chemistry lies at the heart of the answers to each of these questions: it can provide analytical techniques that give early warning of emerging problems, recognition of their origins, and access to alternative products and processes to ameliorate undesired impacts. (See Section V-A.)

Continued Economic Competitiveness

The value of U.S. chemical sales is near $175 billion, and we have a positive balance of trade. Preservation of our quality of life depends significantly upon maintaining this position of leadership. Our future competitiveness will be dependent upon staying in the vanguard as the frontiers of chemistry change and upon supplying to industry a stream of talented young scientists who have been working at these frontiers and using state-of-the-art instrumentation. (See Section V-B.)

Increased National Security

Key factors underlying national security are a healthy populace and a dynamic, productive economy. In both spheres, chemistry plays an essential role. In addition, the nation must be able to deter armed conflict. Again, chemistry is a vital contributor; it enters all areas of defense from propulsion, weapons materials, and classical munitions to the most advanced strategic concepts. (See Section V-C.)

INTELLECTUAL FRONTIERS IN CHEMISTRY

Fortunately, this is a time of intellectual ferment in chemistry deriving from our increasing ability to probe and understand the elemental steps of chemical change and, at the same time, to deal with molecular complexity. Powerful instrumental techniques are a crucial dimension. We can anticipate exciting discoveries on a number of frontiers of chemistry.

Chemical Kinetics

Over the next three decades, we will see advances in our understanding of chemical kinetics that will match those connected with molecular structures over the last three decades. Lasers by themselves have spectacularly expanded experimental horizons for chemists. They can now probe chemical reactions on a time scale that is short compared to the lifetime of any transient substances that can be said to possess a molecular identity. Elementary reactions can be dissected, first, through detailed control of energy content of reactants and, then, through discrimination of energy distribution and recoil geometry among the products. Pathways for energy movement between and within molecules can now be experimentally tracked and theoretically resolved. These new avenues of study will clarify the factors that govern temporal aspects of chemical change. (See Section III-D.)

Chemical Theory

Chemistry is on the verge of a renaissance because of emerging ability to fold experiment and theory together to design chemical structures with properties of choice. With today's computers, accurate calculations can clarify transient situations not readily accessible to experimental measurements, such as intermediate steps in combustion processes. In some cases benefitting from the power of computers, theoretical understandings are developing across chemistry, including dynamics of reactive collisions, electron transfer reactions in solution, and statistical mechanical descriptions of the liquid state. (See Section III-D.)

Catalysis

Developing insights fueled by an array of powerful instrumentation are now moving catalysis from an art to a science. It is now possible to "see" molecules as they react on catalytic surfaces. Metal-organic compounds with purposeful steric specificity and reactivity can be prepared. Organic molecules with predetermined surface conformations that simulate enzymatic architectures can be synthesized. Coherence is appearing that encompasses surface, solution, electrochemical, photochemical, and enzymatic catalysis. Fundamental advances in these various facets of catalysis are forthcoming that will have great economic and technological impact. (See Sections III-A, III-B, V-D.)

Materials

Modern experimental techniques and chemical principles now permit systematic chemical strategies for discovery and design of novel materials. Hence, chemists are increasingly joining and expanding the specialist communities concerned with glasses, ceramics, polymers, alloys, and refractory materials. Coming years will see entirely new structural materials, liquids with orientational regularity, self-organizing solids, organic and ionic conductors, acentric and refractory materials. Chemists will have a central position on the most dramatic frontier of materials science, the design of molecular-scale memory and electrical circuit devices. (See Sections III-C, V-B, V-D.)

Synthesis

Modern instrumental techniques greatly facilitate discovery and testing of new reaction pathways and synthetic strategies. Our accelerating progress, which extends from invention of new families of inorganic compounds to the synthesis of ever-more complex organic structures, is erasing the border between inorganic and organic chemistry. Reactivity control in metal-organic molecules can now be achieved through insightful choice of molecular appendages; new soluble catalysts will result. Molecules with metal atom clusters at their cores can be synthesized to link the chemistry of bulk metals to that of simple metal-organic compounds. This linkage relates the action of dissolved and surface catalysts. Organic molecules of biological complexity can be structurally identified and precisely replicated; this opens the way to tailored biological function. (See Sections III-D, IV-A, IV-B, IV-D.)

Life Processes

The recent striking advances in biology have exposed problems of revolutionary significance that require analysis in terms of molecular interactions. With its ability to deal with molecular complexity, chemistry can play its role in investigating and clarifying the molecular origins of biological processes.

Working hypotheses for biological functions can be tested through deliberate synthesis of tailored molecules: natural product analogs, chemotherapeutic agents, proteins deliberately altered to provide new functions, genetic inserts. This will move us closer to real understanding of the basic workings of life processes in response to the strongest of human preoccupations, the nature and preservation of life. (See Sections IV-B, IV-C, IV-D, V-B.)

Analytical Methods

Conceptual advances in detection, characterization, and quantification of chemical species are benefitting chemistry and contiguous sciences on many fronts. Incorporation of computers is a key factor. Analytical separations based on a variety of chromatographic techniques are essential elements of the rapid progress in identification and synthesis of natural products. Novel ionization methods extend mass spectrometry to biologic macromolecules and other nonvolatile solids. Surface analysis and electroanalytical methods are helping to clarify important aspects of catalysis. Remote spectroscopic and a variety of laser techniques are furnishing timely contributions to environmental monitoring and protection. (See Sections V-A, V-D.)

PRIORITY AREAS IN CHEMISTRY (See Ch. VII.)

The strength of American science has been built by allowing creative, working scientists to decide independently where the best prospects lie for acquiring significant new knowledge. Many of the most far-reaching developments, both in concept and application, have come from unexpected directions. Thus, to list priority areas carries the risk of closing off or quenching some adventurous new directions with potential yet to be recognized.

Even so, it makes sense to concentrate some resources in specially promising areas. This can be done if we regard our research support as an investment portfolio designed to achieve maximum gain. A significant part of this investment should be directed toward consensually recognized priority areas but with a flexibility that encourages evolution in these areas as new frontiers emerge. A second substantial element in this portfolio should be support of creative scientists who propose to explore new directions and new ideas. Finally, a third element must be provision of the instrumentation and the infrastructure needed to assure the cost effectiveness of the entire portfolio.

Where this balance will fall for each of the funding sources will vary. Industrial research will weight heavily the currently recognized priority frontiers. At the other extreme, NSF must encourage the new avenues from which tomorrow's priority lists will be drawn. The other mission agencies should structure their portfolios between those extremes. With such a balanced portfolio in mind, the following priority areas are identified with the intent to achieve the greatest intellectual and societal returns.

Recommendation 1

Priority should be given to the following research frontiers:

A. Understanding Chemical Reactivity
B. Chemical Catalysis
C. Chemistry of Life Processes
D. Chemistry Around Us
E. Chemical Behavior Under Extreme Conditions

Recommendation 1 should be implemented through initiatives sponsored by the relevant mission agencies, scaled by each agency in its own appropriate balance with its support of creative scientists expected to explore new directions and new ideas.

Initiative A. Understanding Chemical Reactivity

We propose an initiative to apply the full power of modern instrumental techniques and chemical theory to the clarification of factors that control the rates of reaction and to the development of new synthetic pathways for chemical change.

Principal objectives are to sustain international leadership for the United States at the major fundamental frontier of chemistry—control of the rates of chemical reactions—and to provide the basis for U.S. competitive advantage in development of new processes, new substances, and new materials.

Initiative B. Chemical Catalysis

We propose an initiative to apply the techniques of chemistry to obtain a molecular-level and coherent understanding of catalysis that encompasses heterogeneous, homogeneous, photo-, electro-, and artificial enzyme catalysis.

A principal objective here will be to provide the fundamental knowledge and creative manpower required for the United States to maintain competitive advantage in and to develop new catalysis-aided technologies.

Initiative C. Chemistry of Life Processes

We propose an initiative to develop and apply the techniques of chemistry to the solution of molecular-level problems in life processes and to develop young research scientists broadly competent in both chemistry and the biological sciences.

A principal objective of this initiative will be to accelerate the conversion of qualitative biological information into techniques and substances useful in biotechnologies, in human and animal medicine, and in agriculture.

Initiative D. Chemistry Around Us

We propose an initiative devoted to understanding the chemical make-up of our environment and the complex chemical processes that couple the atmosphere, *oceans, earth, and biosphere, with special reference to man's conscious and inadvertent disturbance of this global reactor. Analytical chemistry and reaction dynamics define the core of this initiative.*

Principal objectives of this initiative are to provide the basic chemical understandings needed to protect our environment and to extend detection of potentially hazardous substances well below toxicity bounds so that potential problems can be anticipated and ameliorated long before hazard levels are reached.

Initiative E. Chemical Behavior Under Extreme Conditions

We propose an initiative to explore chemical reactions under conditions far removed from normal ambient conditions. Chemical behaviors under extreme pressures, extreme temperatures, in gaseous "plasmas," and at temperatures near absolute zero provide critical tests of our basic understandings of chemical reactions and new routes toward discovery of new materials and new devices.

Principal objectives are to broaden our understanding of chemical change and to lead to new materials that will have application under extreme conditions of pressure, temperature, and exposure to specially challenging environments (e.g., fusion reactors, reentry vehicle heat shields, superconducting magnets).

EXPLOITING THE OPPORTUNITIES IN CHEMISTRY

The extent to which our nation will be able to benefit directly from these promising frontiers in chemistry is, in part, a matter of resources. This report shows that existing patterns of funding are anachronistic and inadequate. Average grant sizes are too small; for example, the average NSF grant will barely support the research activities of two or three students, while an active research group might range in size from six to sixteen (see Table VII-8 and the discussion preceding it). Furthermore, the grants do not provide support for the infrastructure needed to sustain the sophisticated scientific activities of today's chemistry (electronic, computer, and laboratory technicians, machinists and glass blowers, supplies). The inadequacy of support for "mid-cost" instrumentation (less than $1 million), both for shared use among several research groups and for specialized and dedicated use, requires painful trade-offs that tend to restrict capacity to fund new, young investigators entering chemistry. (See Figure, p. 302.) The instrumentation crisis is exacerbated because university chemistry departments are struggling to provide the operating and maintenance infrastructure needed to use this state-of-the-art equipment with maximum cost effectiveness. (See Tables VII-4 and VII-6.)

The listing of opportunities and potential rewards to society that will flow from them is impressive. That we cannot afford to lose these rewards is underscored by the economic importance of chemistry. Business and industry employ more doctoral chemists than the sum of those employed in the biological sciences, mathematics, physics, and astronomy combined (see Appendix Table A-4). Yet we find that the average federal investment in the crucial human resource in chemistry is only one fifth as much per Ph.D. as in other comparably important disciplines (see Table VII-1). Without a more determined U.S. commitment to the chemical sciences, there is substantial likelihood that our leadership position will be preempted abroad.

Chemistry in Industry

The Chemistry and Allied Products industry invests heavily in its own in-house research. This report should be of value to the industry as it decides upon the amount and focus of its own research investment. In addition, U.S. industry has an interest in the health and direction of university-based fundamental research. Industrial progress and competitiveness also depend upon access to a reservoir of fundamental knowledge constantly replenished by university-based research and upon a stream of talented young scientists familiar with the latest chemical frontiers and instrumental techniques. Hence, industry furnishes direct support to university research. Though modest in total (about $10 million each to chemistry and chemical engineering in 1983), it is extremely important because it facilitates movement of new discoveries into new applications and influences university research agendas.

Recommendation 2

New mechanisms and new incentives should be sought for strengthening links between industrial and academic research.

Recommendation 3

Industry should increase its support for university fundamental research in the chemical sciences. Tax incentives to encourage such gains should be explored.

The Federal Role in Fundamental Research

Industry can engage in only a modest amount of the most fundamental and adventurous research because the time horizon for application is remote. Yet, this "high-risk" research offers the most far reaching benefits to society and the intellectual basis for technological competitiveness. It is an appropriate place for public investment.

This report displays an array of opening research frontiers rich in potential for societal benefit. In this setting, an examination of funding patterns in a variety of disciplines that depend upon sophisticated instrumentation reveals that the federal investment in chemistry is not adequate and will not bring to society the full benefits to be realized.

Recommendation 4

The federal investment in chemistry should be raised to be commensurate with the practical importance of chemistry, both economic and societal, and with the outstanding intellectual opportunities it now offers.

Chemistry and the NSF Mission

Chemistry supported by the NSF is judged on its potential for adding to our understanding of nature. Since the most far-reaching technological changes tend to stem from unpredictable discoveries, the fundamental research supported by NSF is critical to this country's long-range technological future. The increasing dependence of our economy upon the health of our chemical industry coupled with the exciting intellectual opportunities in chemistry justify a considerably larger NSF support in all three of its crucial dimensions: shared instrumentation, dedicated instrumentation, and grant size. Such support is needed to assure a U.S. position of international leadership in the exploitation of the rich opportunities before us.

Recommendation 5

(a) NSF should begin a 3-year initiative to increase its support for chemistry by 25 percent per year for FY 1987, FY 1988, and FY 1989.

(b) The added increments should be directed toward increasing grant size, ensuring encouragement of young investigators, enhancing the shared instrumentation program, and increasing the amount directed toward dedicated instrumentation.

(c) NSF should build into its shared instrumentation program a federal capital investment averaging at 80 percent of instrument cost together with maintenance and operating costs for a 5-year period after purchase.

Chemistry and the Department of Energy Mission

For at least the next quarter century, 90 percent of our still growing energy use must come from chemical energy sources. At the same time, the quality and character of feedstocks will be changing in ways that challenge existing technologies and that make it harder to resolve society's concerns about environmental pollution. To meet these challenges, the Department of Energy currently invests in its Chemical Sciences Program only 5 percent and in its Biological Energy Research Program less than 1 percent of the total resources it directs toward 11 of its largest fundamental research programs. To assure our future access to abundant and clean sources of energy over the next three decades, DOE must make a much larger commitment to the chemical sciences. This commitment must engage more fully both the DOE National Laboratories and the larger chemistry community.

Recommendation 6

(a) The DOE should establish a major initiative in those areas of chemistry relevant to the energy technologies of the future.

(b) In an appropriate number of our National Laboratories, the defined mission should be reshaped to include a major focus on one or more of the chemistry areas crucial to energy technologies.

(c) University research programs in energy-relevant areas of chemistry should be raised to be commensurate with those in the National Laboratories.

(d) Incremental growth in these programs by a factor of about 2.5 will be needed to exploit the opportunities before them. For cost effectiveness, this growth should be uniformly spread over the next 5 years. A $22 million incremental growth in the FY 1986 DOE chemistry budget would support an appropriate beginning.

Chemistry and the NIH Mission

Progress in both medicine and chemistry now makes it possible to interpret complex biological events at the molecular level. Because of the ubiquitous role of chemistry in human health, NIH provides substantial support to chemists engaged in research at the broad interface of physiology/medicine/chemistry. Chemistry research relevant to the NIH mission concentrates largely in the Institute for General Medical Sciences. Characteristically, the grants are modest in size, and the award success rates have fluctuated widely over the last decade.

Recommendation 7

(a) A fraction of any additional NIH funds in support of chemistry should be used to increase average grant size, including grants for young investigators and particularly for cross-disciplinary collaborative programs that link expertise in chemistry with that in other health-science disciplines.

(b) NIH should vigorously continue its attempts to stabilize its extramural grant program.

(c) NIH should maintain its extramural shared instrumentation program at a level approximately equal to that of NSF. The initial federal capital investment should include at least 80 percent of instrument cost, and maintenance and operating costs should be provided for 5 years after purchase.

Chemistry and the Department of Defense Mission

Chemistry plays a critical part in our national security. It not only strengthens our ability to deter and prevent armed conflict, it also contributes strongly to the health of the economy and to the maintenance of the technical manpower pool needed to develop and deploy our increasingly sophisticated defense technologies. In the longer view, our future national security, our international economic posture, and our technical manpower supply dictate DOD attention to fundamental chemical research, including that conducted at universities. Yet DOD support of fundamental research has grown very little over the last 5 years; its investment in university research does not fulfull DOD's desire to maintain our manpower pool while providing indirect influence on university research agendas toward promising chemistry areas key to our defense posture.

Recommendation 8

(a) The percentage of the DOD R&D budget directed to basic (6.1) research should be increased to restore the 1965 value of 5 percent.

(b) DOD support for university research in the chemical sciences should be raised to about 25 percent of the total federal support for basic research through real growth at 10 percent per year.

(c) Parallel growth should be provided to DOD in-house research programs of the 6.1 category in chemistry.

(d) Growth should concentrate attention on the special opportunities now offered through chemistry in the following broad research areas:

—Strategic and critical materials
—Fuels, propellants, and explosives
—Atmospheric phenomena
—Chemical and biological defense
—Nuclear power and nuclear weapons effects

(e) Interaction between DOD laboratories and universities should be encouraged and increased.

(f) DOD should continue its instrumentation program but with the addition of support for maintenance and operation.

(g) DOD should explore mechanisms to support new construction and renovation of university research facilities in particularly critical areas of chemistry.

Chemistry and the Department of Agriculture Mission

The USDA devotes only a small portion of its R&D budget to chemistry research relevant to agriculture and animal health. But human needs are great, so we can ill afford to miss the relevant opportunities offered by chemistry.

Recommendation 9

> **The Department of Agriculture should initiate a substantial competitive grants program in chemistry with the aim of increasing extramural support of fundamental research in chemistry relevant to agriculture and animal health to an approximate par with the Department's intramural program.**

Chemistry and the National Aeronautics and Space Administration Mission

The several initiatives proposed in this report present opportunities for improvement of the safety, range, and effectiveness of future space operations. Furthermore, NASA has unique capabilities for monitoring and mission-related concern about the changing chemical compositions of the troposphere and the stratosphere.

Recommendation 10

> **(a) The National Aeronautics and Space Administration should maintain a substantial commitment to the understanding of atmospheric chemistry.**
>
> **(b) Increased attention should be directed toward special opportunities relevant to operations in space:**
>
> **—high energy propellants;**
> **—chemical behavior under extreme conditions;**
> **—reaction kinetics and photochemistry under collision-free conditions.**
> **—chemical aspects of life-sustenance in a closed system**
> **—analytical methods for compositional monitoring in both the troposhere and the stratosphere**
>
> **(c) NASA should more actively encourage academic chemists to address problems relevant to the NASA mission through competitive grants for fundamental research.**

Chemistry in the Environmental Protection Agency

The EPA has significant R&D programs specifically and properly directed toward currently recognized environmental problems, and many of these programs involve chemistry. This agency assumes a much less active role in fundamental research relevant to its mission as epitomized by its tiny Exploratory Research program. This extramural program is now funded at $16M, less than 0.4 percent of the $4.25B EPA total. The EPA should follow the pattern of other federal mission agencies by defining those areas of research that underlie its mission goals and stimulating the expansion of knowledge in those areas through programs of fundamental research.

Recommendation 11

(a) EPA should increase the percentage of its R&D funds placed in its Exploratory Research program and its commitment to extramural fundamental research relevant to environmental problems of the future.

(b) EPA should encourage fundamental chemical research to clarify reaction pathways open to molecules, atoms, and ions of environmental interest.

(c) EPA should take a prominent role in support of long-range research in analytical chemistry with emphasis on extension of sensitivity limits, increase in detection selectivity, and exploration of new concepts.

(d) EPA should have as a conscious and publicized goal the detection of potentially undesired environmental constituents at concentration levels far below known or expected toxicity limits.

Conclusion

In the next two decades there will be dramatic changes in our basic understanding of chemical change and in our ability to marshal that understanding to accomplish deliberate purpose. The program presented here defines a leadership role for the United States as these advances are achieved. The rewards accompanying such leadership are commensurate with the prominent role of chemistry in addressing society's needs, in ameliorating problems of our technological age, and in sustaining our economic well-being. The costs of falling behind are not tolerable.

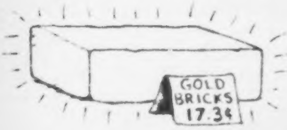
VIGNETTES

Chemistry is a central science that responds to societal needs. It is critical in man's attempt to discover new processes, tap new sources of energy, develop new materials, feed the world's population, improve health and conquer disease, and monitor and protect our environment.

Beauty Is Only Skin Deep

Ever think of going into the gold-brick business? Just take a big hunk of gold and a hacksaw and you've got a good-looking brick with a nice heft. Unfortunately, one such brick and you're talking $140-150,000! There's no room for mark-up. But suppose you get an ordinary brick (wholesale in South Jersey, 17¢!) and just coat the surface with gold—the cost will come down a lot. And you'll have a beautiful brick—well, at least "skin-deep."

So how much would such a surface coating cost? For openers, put a one-atom-thick layer of gold atoms over the entire surface of the brick. Let's see, 2 inches by 4 inches by 8 inches—gold at $320 an ounce—one atom thick—that'll be . . . 0.3¢ worth of gold. Wow! There, we've got an attractive product at a total material cost of 17.3¢ (not including packaging).



That's pretty impressive. It means that the outermost layer (the surface) of a $150,000 piece of gold involves so few atoms that they would cost less than a cent. Yet that miniscule fraction of atoms on the surface of a piece of metal controls the chemistry of that piece. For instance, these surface atoms are the ones that determine whether the metal surface acts as a catalyst or not. And catalysts account, one way or another, for about 20 percent of our gross national product.

So what is a catalyst? It's a chemical substance that speeds up a chemical reaction without itself getting into the act (i.e., it is not consumed while doing its thing). A solid catalyst merely furnishes its surface as a meeting place for gaseous molecules. For instance, when a molecule of methanol lands on a rhodium catalyst surface, it usually sticks for a while (becomes adsorbed). Now, if a carbon monoxide molecule happens to arrive, zingo, it reacts with an adsorbed methanol molecule and they leave the surface as acetic acid. When methanol and carbon monoxide meet in the gas phase, they won't even give each other the time of day. But because of the special environment provided by that thin layer of surface atoms on the rhodium catalyst, methanol and carbon monoxide react so rapidly that 500,000 tons of commercial acetic acid are made every year this way! This kind of speed-up might be anywhere from a thousand-fold to a million-fold when things are working.

Because of such successes, chemists care a lot about how these catalytic gold bricks do their job. What actually happens to that thin layer of adsorbed molecules as they come and go on a catalytic metal surface? Unfortunately, that's where the skin-deep principle works against us. If there isn't much on the surface, there isn't much to see.

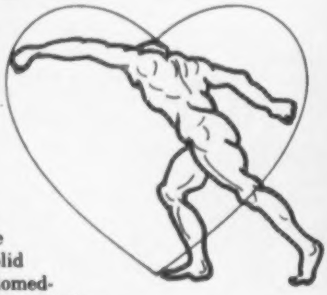
But nowadays, we have several powerful instruments with which we can learn about the special properties of the skin of a metal. These instruments also let us watch molecules as they lodge on the surfaces of catalysts like platinum and rhodium and many others. We can see how the molecules are chemically changed by the metallic skin to make them more reactive when a suitable reaction partner comes along. So chemists are beginning to understand how to design these catalytic gold bricks to do whatever we want. Right now, every gallon of your gasoline began as a bunch of molecules sure to make your engine knock and then some chemist catalytically converted them into other molecules that make your engine purr. But now we are looking ahead to new energy feedstocks with more sulfur and metallic contaminants that will require much better catalysts so that we can keep your engine purring and the air clean at the same time. We'll do it by learning how those catalytic gold bricks work so we can tailor them to our needs. This is a case where skin-deep beauty really pays off!

A Lithium-Powered Heart

The cardiac pacemaker is a modern miracle of science that many of us take for granted—but not a person who owns one! These pacemakers operate on battery power, and the demands put on the tiny batteries that generate it are awesome! They must start the human machine every morning *without fail*, and the human lights and radio are running all the time. Yet many, many people are adding healthy years to life by betting on the chemical reactions that occur in these batteries to generate—day in, day out—the electric current that drives their pacemakers.

These batteries have special requirements because they must be implanted in a human body. They must be rugged and leakproof, have long life and minimal weight, and, of course, they must be nontoxic. The first batteries used in pacemakers had a lifespan of only 2 years, and the periodic operations required for replacement meant additional risk and stress for the patient.

Chemists began to tackle this problem, and research efforts in electrochemistry uncovered lithium metal, a new substance with the potential to give long life to batteries. Unfortunately, lithium is highly reactive—it burns in air and reacts with water to produce flammable hydrogen gas. If lithium were to be used, it would be necessary to discover new, nonaqueous electrolyte systems.



Electrolytes are substances that dissolve in water to form conducting solutions. They dissolve to produce ions, particles carrying electrical charge. The movement of these charges carries the current as the battery's chemistry releases its stored energy. Conventional batteries that draw on the chemical energy of zinc and mercuric oxide depend upon aqueous electrolytes. So the problem for the chemists to solve was defined—to design a battery that would operate without water.

Extensive investigations into new solvents and new materials for use in high energy, long-life batteries eventually led to the discovery of a solid electrolyte for use with lithium metal. The solid electrolyte is iodine, and the lithium-iodine battery was born for biomedical applications. These revolutionary batteries are currently in use, and they have an impressive lifespan of 10 years! The benefits to those who must depend upon cardiac pacemakers are incalculable.

The lithium-iodine battery is not the end of the story. It is a vast improvement over its predecessors and extremely useful in pacemakers, but it has a lower power than would be optimum for other uses. On the horizon is the need for new, higher-power batteries for use in other implantable organs like artificial kidneys and hearts. But further electrochemical research will undoubtedly provide the answer. It has in the past, and it will again.

Stone Age, Iron Age, Polymer Age

There was a time when everything from arrowheads to armchairs was made from stones. Other features of those good old days were air-conditioned caves and charbroiled saber-toothed tiger steaks (if you caught him instead of the other way around). Fortunately, this age ended when someone discovered how to reduce iron oxide to metallic iron using coke (carbon) as the reducing agent. That all happened several thousand years ago, so the caveperson chemist who got the patent rights to the iron age wasn't educated at MIT or the University of Chicago. But this chemical discovery profoundly changed the way people lived. It led to all sorts of new products like swords and plowshares and the inner-spring mattress. Can you imagine how those stone-agers would have reacted the first time they put on a suit of armor, or went up the Eiffel Tower, or took the train to Chattanooga? Well, brace yourself, because chemists are at it again! This time, we're about to enter the Polymer Age.

NEXT TRAIN
CHATTANOOGA

You may think we're already there, with your polyester shirt, polyethylene milk bottle, and polyvinylchloride suitcase. We walk on polypropylene carpets, sit on polystyrene furniture, ride on polyisoprene tires, and feed our personal computers a steady diet of polyvinylacetate floppy disks. In just the last 40 years, the volume of polymers produced in the United States has grown 100-fold and, since 1980, actually exceeds the volume of iron we produce. But the best is yet to come.

The structural materials with which we have been building bridges since even before the one to Brooklyn, and automobiles since the Model T, would seem to be the last stronghold of the iron age (pun intended). Would anyone dare to suggest that polymers could compete on this sacred ground? Well, no one perhaps except chemists. Right now, there's talk of an all-plastic automobile, and you're already flying in commercial airliners with substantial structural elements made of composite polymers. One of these, poly(para-phenylene terephthalamide), has a tensile strength slightly higher than that of steel. But where this polymer really scores is in applications where the strength-to-weight ratio matters a lot, as it does in airplanes. Even with its cumbersome name, this polymer has a strength-to-weight ratio six-fold higher than steel! To appreciate this advantage, you should know that a 1-pound reduction in the structural weight of an airplane reduces its take-off weight by 10 pounds (counting the fuel to lift the pound and the fuel to lift the extra fuel). No wonder this polymer, under the trade name Kevlar®, is used to build tail sections for the biggest airliners. Oh, and bullet-proof vests, too.

So what about this all-plastic automobile? Of course, weight reduction is the name of the game in trying to build fuel-efficient cars. Already there are automobile driveshafts made of polymers strengthened with stiff fibers, and similar composites are used for leaf springs (oops, there goes the inner-spring mattress!). Right now, U.S. cars contain about 500 pounds of plastics if you count, as well, the rubber and paint and sealants and lubricants and upholstery.

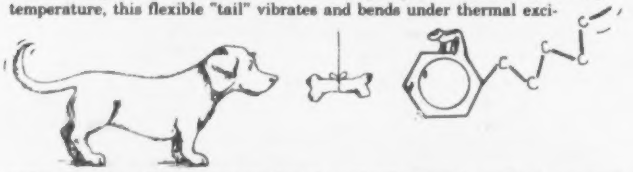
But what about the engine and the electrical system? What will we do about these in this allegedly all-polymer car? Gee, I'm glad you asked. . . .

The Time It Takes to Wag a Tail

When your pet dog sniffs a bone, instantly his tail begins to wag. But it must take *some* time for the northernmost canine extremity to send the news all the way south where enthusiasm can be registered! How long does it take for that delicious aroma to lead to the happy response at the other end? Chemists are now asking the questions much like this about their pet molecules! If one end of a molecule is excited, how long does it take for the other end to share in the excitement? That time may determine whether the excitation will result in a chemical reaction in the part of the molecule where the energy was injected, somewhere else, or nowhere at all.

For the canine experiment, we need a hungry dog, a quick hand with the bone, and a quick eye to read the stopwatch. For molecules, it's much harder. Only within the last few years has it been possible to measure the rate of energy movement within a molecule. But chemists now have pulsed lasers giving bursts of light with durations as short as a millionth of a millionth of a second (a "picosecond"). Comparing a chemical change that takes place in 1 picosecond to a 1-second tail-wag delay involves the same speed-up as a 10-second instant replay of all historical events since the pyramids were built.

The alkyl benzenes provide an example. Each of these molecules has a rigid benzene ring at one end and a flexible alkyl group at the other. At room temperature, this flexible "tail" vibrates and bends under thermal excitation. But to act like our hungry dog, the molecules must be cooled to cryogenic temperatures, while avoiding condensation. Supersonic jet expansion makes this possible. When a gas mixture flows through a jet nozzle into a high vacuum, the molecules can be cooled almost to absolute zero. An alkyl benzene molecule carried along in such a stream loses all its vibrational energy, thus relaxing the molecular tail. Then, the cold molecules intersect a brief pulse of light with color that is absorbed by the benzene ring. With careful "color-tuning," extra vibrational energy can be placed in the head without any vibrational excitation in the tail. Then we must watch the molecule to see how long it takes for the tail to wag. Fluorescence lets us do this.



When a molecule in a vacuum absorbs light, the only way it can get rid of the energy is to re-emit light. Such fluorescence can be recorded with a fast-response detection system to give a spectrum that carries a tell-tale pattern showing where the extra energy was at the instant the light was emitted. Those molecules that happen to emit right away after excitation show the molecular head vibrating and the tail still cold. Those that emit later have an emission spectrum that shows that the tail is wagging. In this way, we have learned that the time it takes for the alkyl benzene tail to begin to wag depends on how long the tail is. Surprisingly, the longer the alkyl, the faster the movement out of the ring. The result shows what determines energy flow within molecules (the "density of states"). Such information might one day clarify combustion and help us make fine chemicals out of coal.

Whipping a Wicked Weed

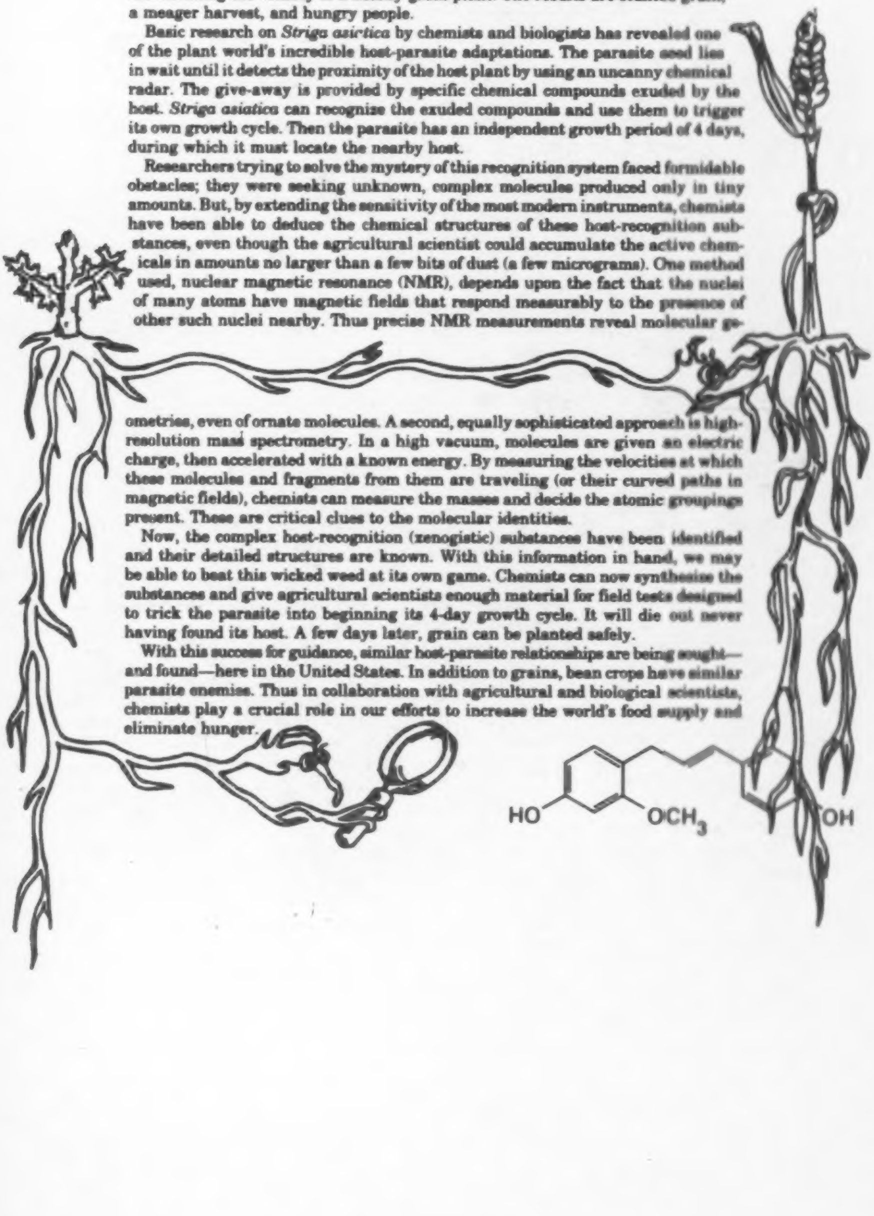
The plant *Striga asiatica* is one of the most devastating destroyers of grain crops in the world. This wicked weed restricts the food supply of more than 400 million people in Asia and Africa. It is a parasite that nourishes itself by latching onto and draining the vitality of a nearby grain plant. The results are stunted grain, a meager harvest, and hungry people.

Basic research on *Striga asirtica* by chemists and biologists has revealed one of the plant world's incredible host-parasite adaptations. The parasite seed lies in wait until it detects the proximity of the host plant by using an uncanny chemical radar. The give-away is provided by specific chemical compounds exuded by the host. *Striga asiatica* can recognize the exuded compounds and use them to trigger its own growth cycle. Then the parasite has an independent growth period of 4 days, during which it must locate the nearby host.

Researchers trying to solve the mystery of this recognition system faced formidable obstacles; they were seeking unknown, complex molecules produced only in tiny amounts. But, by extending the sensitivity of the most modern instruments, chemists have been able to deduce the chemical structures of these host-recognition substances, even though the agricultural scientist could accumulate the active chemicals in amounts no larger than a few bits of dust (a few micrograms). One method used, nuclear magnetic resonance (NMR), depends upon the fact that the nuclei of many atoms have magnetic fields that respond measurably to the presence of other such nuclei nearby. Thus precise NMR measurements reveal molecular geometries, even of ornate molecules. A second, equally sophisticated approach is high-resolution mass spectrometry. In a high vacuum, molecules are given an electric charge, then accelerated with a known energy. By measuring the velocities at which these molecules and fragments from them are traveling (or their curved paths in magnetic fields), chemists can measure the masses and decide the atomic groupings present. These are critical clues to the molecular identities.

Now, the complex host-recognition (xenogistic) substances have been identified and their detailed structures are known. With this information in hand, we may be able to beat this wicked weed at its own game. Chemists can now synthesize the substances and give agricultural scientists enough material for field tests designed to trick the parasite into beginning its 4-day growth cycle. It will die out never having found its host. A few days later, grain can be planted safely.

With this success for guidance, similar host-parasite relationships are being sought—and found—here in the United States. In addition to grains, bean crops have similar parasite enemies. Thus in collaboration with agricultural and biological scientists, chemists play a crucial role in our efforts to increase the world's food supply and eliminate hunger.



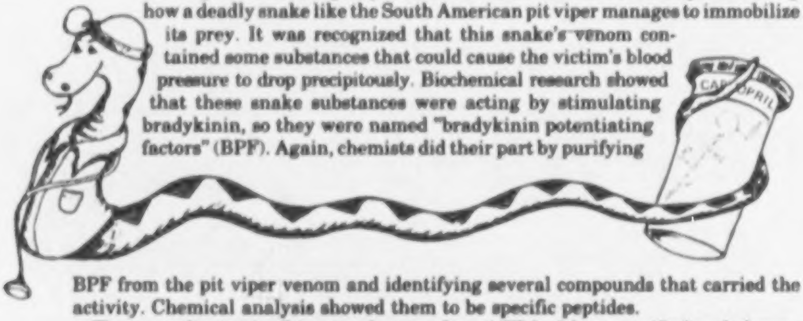
R_x-Snake Bite

High blood pressure, anyone? Maybe you'd like a dose of snake venom? Yes, it's true! Hypertension sufferers may find their future treatment coming from this unlikely source—and from sustained research in chemistry and physiology.

This story began 30 years ago when scientists discovered the chemical mechanism by which blood pressure is elevated in humans. Chemical techniques isolated two closely related substances, angiotensin I and angiotensin II. In the human body chemistry, II is produced from I with the help of a specific enzyme, "angiotensin-converting enzyme" (ACE). Though I has no physiologic effect, its reaction produced *angiotensin II*, the most potent blood pressure-elevating substance known. Thus I provides a reservoir from which II can be made as needed to maintain a normal blood pressure level, a conversion controlled by the enzyme ACE.

It is no surprise that there is also a substance provided by Nature to lower blood pressure—this substance is called *bradykinin*, which, along with angiotensin II, seems to complete the control mechanism. To raise pressure when it is too low, make some angiotensin II using ACE. To lower blood pressure when it is too high, a dash of bradykinin will do the trick.

During the 1960s, a group of Brazilian scientists were bent upon learning how a deadly snake like the South American pit viper manages to immobilize its prey. It was recognized that this snake's venom contained some substances that could cause the victim's blood pressure to drop precipitously. Biochemical research showed that these snake substances were acting by stimulating bradykinin, so they were named "bradykinin potentiating factors" (BPF). Again, chemists did their part by purifying BPF from the pit viper venom and identifying several compounds that carried the activity. Chemical analysis showed them to be specific peptides.



The next chapter in this story began when ACE had been purified and characterized. That opened the door to understanding how the snake venom BPF did its work. Some of the peptides in BPF block ACE to interfere with the production of angiotensin II. Then, as a bit of a surprise, it was discovered that ACE derived part of its control function from an ability to inactivate bradykinin. Realizing this, the canny pit viper provides some peptides in its venom to protect bradykinin from inactivation! Thus these BPF peptides deprive the body of its ability to use ACE, either to raise blood pressure by producing angiotensin II or to moderate the lowering action of its own control substance, bradykinin.

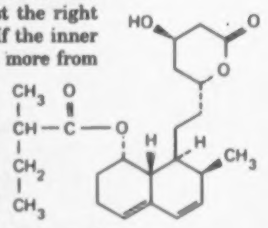
With this understanding, teams of biologists and chemists recently began a systematic attack on hypertension, one of the most insidious causes of death in our stressful world. They synthesized a series of peptides modeled on those found in snake venom but designed for therapeutic use. Success came with the synthesis of the compound captopril. It acts as an ACE inhibitor, and clinical trials have amply demonstrated its ability to lower abnormally high blood pressure. No wonder that the medical profession has great expectations for ACE enzyme inhibitors in the treatment of our hypertensive population.

A Pac-Man for Cholesterol

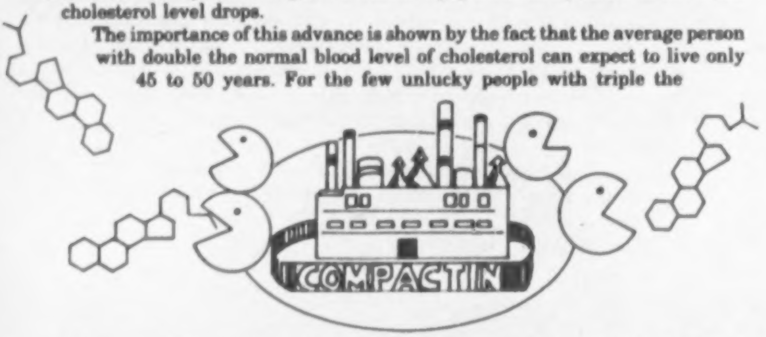
Since the 1960s we've known that high levels of cholesterol correlate with heart ailments, the major cause of death in the United States. What we need is a Pac-Man to chomp up the cholesterol in the blood and reduce "hardening" of the arteries that carry blood from the heart (atherosclerosis). Now a lowly fungus—not unlike the famous mold, penicillium—may have shown us one.

A normally functioning human cell uses a dual system for meeting its cholesterol needs. First, the cell has its own factory to manufacture cholesterol. In addition, the cell's exterior has a number of lipoprotein receptors that can grab onto cholesterol-containing lipoproteins as they pass by in the bloodstream and pull them inside. The cell fixes the number of these Pac-Man-like receptors so that just the right amount of imported cholesterol is added to the factory-made product. If the inner cell cholesterol level falls too low, more receptors are added to extract more from the blood stream.

There's an idea! If the cell's cholesterol factory could be slowed down, would the cell produce more receptors to make up the difference from the blood stream supply? A chance to test this scenario came when a biochemist discovered that certain fungi produced something that inhibited cholesterol synthesis. Chemists joined in the plot, purified the effective compound, determined its structure, and named it COMPACTIN. Knowing this structure, chemists were able to synthesize close relatives of compactin that are even more potent. Chemical tests with these new chemicals indicate that the scheme works as planned. The inhibitor slows down the cellular cholesterol factory, the cell produces more lipoprotein receptors, and the blood cholesterol level drops.



The importance of this advance is shown by the fact that the average person with double the normal blood level of cholesterol can expect to live only 45 to 50 years. For the few unlucky people with triple the normal amount, life expectancy drops to 30 or 35 years. To complicate matters, 1 in 500 Americans has the genetic disease *familial hypercholesterolemia* (FH). Victims of FH don't produce enough receptors at their cell surfaces, so lipoproteins accumulate in the blood and eventually cause heart attacks. Thus clinical researchers are excited to find that the new cholesterol-inhibitors work with FH patients, bringing the blood levels of cholesterol all the way down to the normal level. Much research remains, but these moldy chemicals offer immediate hope to FH sufferers and, in the future, to all people with abnormally high blood cholesterol.



Jack and the Soybean Stalk

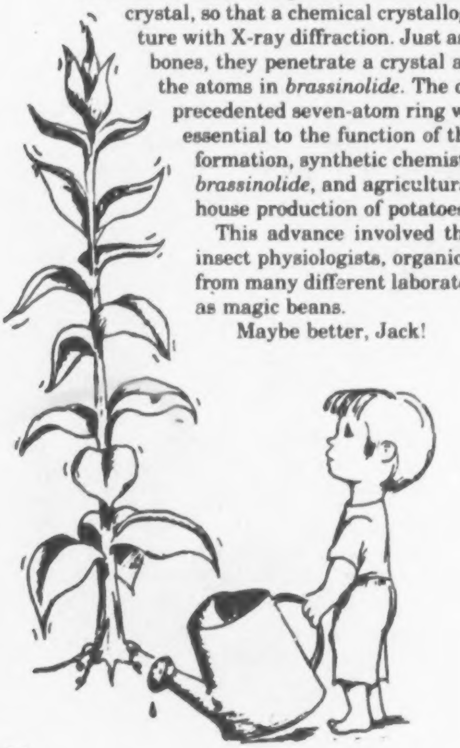
Perhaps a modern explanation for the amazing size of Jack's fairytale beanstalk can be found in *brassinolide*. This remarkable chemical is an extremely effective plant hormone that can double the growth of food plants, by both cell elongation and cell division. Only recently have chemists been able to isolate, identify, and then synthesize this valuable substance so that it can be used to increase the world's food supply.

Plant hormones have already revolutionized agriculture. They allow us to coerce cotton plants to release their cotton balls at harvest time, command fruit trees to cling to their fruit, induce Christmas trees to keep their needles, and order stored potatoes not to sprout. *Brassinolide* now can add to this list, and it is active in quantities of less than one-billionth of an ounce!

Chemists play a crucial role along the long and arduous research road from discovery to use of a new plant hormone. For example, *brassinolide* is found in minute quantities in the pollen of the rape plant (*Brassica rapus* L.). To isolate enough chemical to study, researchers laboriously collected pollen brushed off the legs of bees who had been cavorting in the rape plants. From 500 pounds of pollen so gathered, chemists were able to extract only 15 milligrams of *brassinolide*, an amount as small as a grain of sand. From this they were able to grow a single tiny crystal, so that a chemical crystallographer could analyze the molecular structure with X-ray diffraction. Just as X-rays penetrate an arm to reveal broken bones, they penetrate a crystal and reveal the geometrical arrangement of the atoms in *brassinolide*. The chemists were surprised to discover an unprecedented seven-atom ring within the molecule, a feature that must be essential to the function of this beneficial compound. With this key information, synthetic chemists have now made several close relatives of *brassinolide*, and agricultural scientists are evaluating them in greenhouse production of potatoes, soybeans, and other vegetables.

This advance involved the knowhow and interaction of plant and insect physiologists, organic chemists, and chemical crystallographers from many different laboratories. It shows that mental effort is as good as magic beans.

Maybe better, Jack!



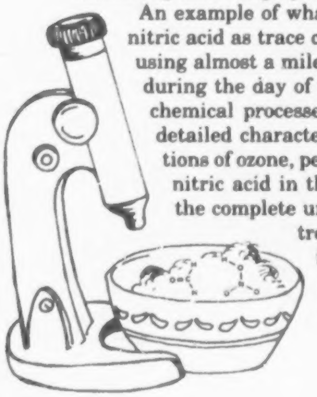
Investigating Smog Soup

Air pollution is a visible reminder of the price we sometimes pay for progress. Emissions from thousands of sources pour into the atmosphere a myriad of molecules that react and re-react to form a "smog soup." We are already aware of some of the potential dangers of leaving these processes unstudied and unchecked: respiratory ailments, acid rain, and the greenhouse effect. Surprisingly, you and I are the principal culprits in generating much of this unpleasant brew—everytime we start our cars or switch on our air conditioning or central heating! Transportation, heating, cooling, and lighting account for about two-thirds of U.S. energy use, almost all derived from combustion of petroleum and coal.

Pinpointing cause and effect relationships begins, inevitably, with the identification and measurement of what is up there, tiny molecules at parts-per-billion concentrations in the mixing bowl of the sky. Finding out what substances are there, how they are reacting, where they came from, and what can be done about them are all matters of chemistry. The first two questions require accurate analysis of trace pollutants. Physical and analytical chemists have successfully applied to such detective work their most sensitive techniques. An example is the *Fourier Transform Infrared Spectrometer*. This sophisticated device can look through a mile or so of city air and identify all the chemical substances present and tell us their concentrations down to the parts-per-billion level. Recognizing a substance at such a low concentration is comparable to asking a machine to recognize you in a crowd at a rock concert attended by the entire U.S. population.

How does this superb device work? "Infrared" means light just beyond the red end of the rainbow visible to the human eye. Hence infrared light is invisible, though we can tell it is there by the warmth felt under an infrared lamp. But molecules can "see" infrared light. Every polyatomic molecule absorbs infrared "colors" that are uniquely characteristic of its molecular structure. Thus each molecular substance has an infrared absorption "fingerprint"—different from any other substance. By examining these fingerprints, chemists can identify the molecules that are present.

An example of what can be done is the measurement of formaldehyde and nitric acid as trace constituents in Los Angeles smog. Unequivocal detection, using almost a mile-long path through the polluted air, revealed the growth during the day of these two bad actors and tied their production to photochemical processes initiated by sunlight. Continuing experiments led to detailed characterization of the simultaneous and interacting concentrations of ozone, peroxyacetyl nitrate (PAN), formic acid, formaldehyde, and nitric acid in the atmosphere. These detections removed an obstacle to the complete understanding of how unburned gasoline and oxides of nitrogen leaving our exhaust pipe end up as eye and lung irritants in the atmosphere. This advance doesn't eliminate smog soup, but it is a big step toward that desirable end.



Libraries into Space

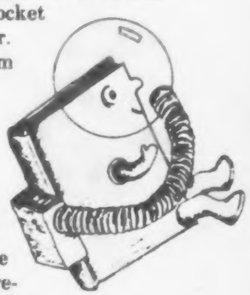
Unbelievable though it sounds, we may have to place whole libraries in a space-like environment over the next decade! This strange proposal is not made because our orbiting astronauts need more reading material, but because if we don't do that or something similar, most of our books won't be around very long for the rest of us to read. An alarming and little known problem faces mankind today—the vast majority of books, those printed since the 1850s, are relentlessly yellowing and crumbling to dust. The library at the University of California at Berkeley alone stands to lose 60,000 books and periodicals per year to decomposition. This is not because of air pollution; the source of the destruction lies in the very paper on which the books are printed. Now, some clever chemists have discovered that, surprisingly, a trip into an environment similar to space provides at least one solution to this vexing problem.

Papermaking processes used since the 1850s universally employ an alumrosin sizing to keep ink from "feathering" or spreading on the paper. Slowly, this papermaker's alum—aluminum sulfate—combines with moisture in the pages and in the air to form sulfuric acid. This aggressive substance, in turn, facilitates attack on the cellulose fibers in the paper, breaking them into smaller and smaller fragments and, ultimately, to dust. Between 75 and 95 percent of the deterioration in "modern" paper is caused by such acid attack.

In recent years, chemists have developed a number of acid-neutralizing processes for books. One of these, developed in the Library of Congress research laboratory, suggests that the chemical diethyl zinc may be ideal for the job. Diethyl zinc is a gas, so its molecules can easily permeate even a *closed* book. Once inside, the substance deacidifies each book and then looks ahead to the future by leaving an alkaline residue of zinc oxy-carbonate. This residue, uniformly distributed throughout the paper fibers, protects the book from any future acid attack.

Ironically, this life-saving agent, diethyl zinc, bursts into flame on contact with air and explodes when it touches water. How does a chemist work with a compound that cannot be exposed to air or water? In a deep space environment, of course. A suitable location was found at NASA's Goddard Flight Center, where 5,000 books from the Library of Congress took a simulated flight, not on a rocket into space, but in a laboratory space-simulating vacuum chamber.

First, the books were thoroughly dried by warming under vacuum for about 3 days. Then, with all oxygen removed from the chamber, gaseous diethyl zinc was introduced and allowed to diffuse into the books. As the neutralizing reaction proceeds, harmless ethane gas is produced and pumped away. Then the protective zinc oxy-carbonate is formed. The results have been extremely promising, and, as the technology is perfected, libraries across the nation will be looking to install huge deacidification facilities. These countermeasures, coupled with the new "alkaline reserve" papers now used in modern printing, promise that the precious heritage of the world's libraries, including the enormous Library of Congress, will be preserved for future generations to enjoy, and profit from, just as we do today.



○